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T R E A T I S E

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**TREATISE ON AIR,**  
CONTAINING  
**NEW EXPERIMENTS AND THOUGHTS**  
ON  
**COMBUSTION;**  
BEING A FULL INVESTIGATION  
OF  
**MR. LAVOISIER's SYSTEM;**  
AND PROVING,  
BY SOME STRIKING EXPERIMENTS,  
ITS ERRONEOUS PRINCIPLES;  
WITH  
STRICTURES UPON THE CHEMICAL OPINIONS  
OF SOME  
**EMINENT MEN.**

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BY RICHARD BEWLEY, M. D.

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*Anno 1791.*

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TO THE  
**ROYAL SOCIETY.**

GENTLEMEN,

I Respectfully presume to dedicate this Treatise to you. I would not, in the common language of most dedicators, enlarge upon your learning and merit. These, since you became a ROYAL SOCIETY, have attracted the notice and admiration of Europe; so that, by your skilful and useful researches in the philosophical world, you have risen to the first rank amongst the learned societies. Aerial studies have been, for these last twenty years, the favourite pursuit of learned and ingenious men; and your Society, ardently anxious to carry these pursuits to the utmost, has been foremost in its labours, and most successful in them.

These considerations induce me to present you, as the Guardians of science in this happy and learned isle,  
with

with a theory, which fully accounts for all the aerial phenomena, by a true and accurate investigation of atmospheric air, its formation and purposes. And, though it is in opposition to the opinions of some of your leading and most respectable members; yet, as justice and candour have always been the line of your conduct, I make no doubt, but you will give it an impartial discussion.

It may be thought, that the most regular way would have been, to have sent the sheets to the Society, that they might have been publicly read to the members: This could not be conveniently done, as they are too extensive to have formed a paper for the inspection of the ROYAL SOCIETY.

Need I suggest upon the present occasion, the liberal behaviour of the great Bergman to the immortal Scheele, when he presented to him his theory of heat: For, though it has turned out not to be the true one; yet it merited a fair examination. I am not so

so vain as to compare myself to that eminent chemist; I would only suggest that, the chemical opinions in my Treatise deserve a patient and candid hearing. The system I support, is that of an ingenious chemist, whose labours and himself, have met with uncandid treatment.

The properties and uses of air, are a subject of the utmost importance to science; and, as this system, respecting these, carries all the fair marks of truth, I recommend it to your protection, as to its proper Guardians; not doubting, but your conduct towards it will be, conformable to what is liberal and just, and that future ages will have occasion to applaud your behaviour.

**GENTLEMEN,** *I am,*

*With the most profound Respect,*

*Your most humble Servant,*

**RICHARD BEWLEY.**

LONDON,  
February 12th, 1791.

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soil to which consists of a thin  
layer of topsoil composed of fine  
gravelly sand, with a thin layer of  
loam on top, which is covered by  
a thin layer of fine gravelly sand  
and a thin layer of loam on top.

Soil 2: This soil is composed of a thin  
layer of topsoil composed of fine  
gravelly sand, with a thin layer of  
loam on top, which is covered by  
a thin layer of fine gravelly sand  
and a thin layer of loam on top.  
Soil 3: This soil is composed of a thin  
layer of topsoil composed of fine  
gravelly sand, with a thin layer of  
loam on top, which is covered by  
a thin layer of fine gravelly sand  
and a thin layer of loam on top.

Soil 4: This soil is composed of a thin  
layer of topsoil composed of fine  
gravelly sand, with a thin layer of  
loam on top, which is covered by  
a thin layer of fine gravelly sand  
and a thin layer of loam on top.

Soil 5: This soil is composed of a thin  
layer of topsoil composed of fine  
gravelly sand, with a thin layer of  
loam on top, which is covered by  
a thin layer of fine gravelly sand  
and a thin layer of loam on top.

Soil 6: This soil is composed of a thin  
layer of topsoil composed of fine  
gravelly sand, with a thin layer of  
loam on top, which is covered by  
a thin layer of fine gravelly sand  
and a thin layer of loam on top.

Soil 7: This soil is composed of a thin  
layer of topsoil composed of fine  
gravelly sand, with a thin layer of  
loam on top, which is covered by  
a thin layer of fine gravelly sand  
and a thin layer of loam on top.

Soil 8: This soil is composed of a thin  
layer of topsoil composed of fine  
gravelly sand, with a thin layer of  
loam on top, which is covered by  
a thin layer of fine gravelly sand  
and a thin layer of loam on top.

Soil 9: This soil is composed of a thin  
layer of topsoil composed of fine  
gravelly sand, with a thin layer of  
loam on top, which is covered by  
a thin layer of fine gravelly sand  
and a thin layer of loam on top.



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# T R E A T I S E

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THAT fire when concentrated and fixed, forms phlogiston, is an idea that chemistry has long taught, it being considerably an older doctrine than Sthall's; though they who have adopted the one have likewise the other, there being no contradiction in the union.

It has been the opinion of the first philosophers as well as chemists. Indeed, upon investigation it appears so obvious, that it is almost impossible to think otherwise.

Sir Isaac Newton (though he supposed that light and heat were the general particles of matter put in motion) saw that light was produced from the integral parts of bodies: He says, "Are not gross bodies and light convertible into one another? The

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" change

" change of gross bodies into light, and light  
" into gross, is very conformable to the course  
" of nature, which seems delighted with  
" transmutations."

Dr. Franklin is of the same opinion, who says, " I have been inclined to think that the fluid *fire*, as well as the fluid *air*, is attracted by plants in their growth, and becomes consolidated with the other materials of which they are formed, and makes a great part of their substance; that when they come to be digested, and suffer in the vessels a kind of fermentation, part of the fire, as well as part of the air, recovers its fluid *active* state again, and diffuses itself in the body, digesting and separating it; that the fire so re-produced by digestion and separation continually leaving the body, its place is supplied by fresh quantities, arising from the continual separation; that whatever quickens the *motion of the blood* in an animal, quickens the separation, and produces more of the fire, as exercise; that all the fire emitted by wood and other combustibles when burning, existed in them before in a solid state, being only discovered when separating; that some fossils, as sulphur, sea-coal, &c. contain a great deal of solid fire, and that in short, what escapes and is dissipated in the burning of bodies, besides water and earth, is generally the air and fire that before made parts of the solid."

This

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This same theory has been regularly taught by all chemists; indeed, as Dr. Black observes, if we can believe our senses, we must suppose that heat and light proceed from the evolution of phlogiston.

The late chemists upon air, to counterbalance these great authorities, have adopted some singular theories from their late experiments; so that, instead of following the paths which have been hitherto trodden, they have pursued others in direct opposition.

Dr. Crawford's principles of heat have been lately received; these suppose heat and phlogiston to be two distinct bodies which repel each other; the investigation of which we shall now enter upon.

Dr. Crawford when he first published, made a very considerable difference between the heat that was imparted by pure and impure airs; but upon more accurately attending to the experiments, he found that they were very erroneous: The moisture in the airs when condensed, being the great cause of producing the heat; and, he likewise found, that instead of inflammable air giving little or no heat, that it possesses, agreeable to his experiments, at the rate of 21,4000, while dephlogisticated air only possesses 4,7490.

This is striking against his first principles of caloric and phlogiston repelling each other; so that it appears from his experiments, that the inflammable air, which is principally phlogiston retains so much more heat.

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But as we shall have to observe in other instances, a theory once adopted is tenaciously pursued by its author to the very last extremity. The set of experiments he now gives us, after making many fruitless attempts, and correcting many errors, we shall examine; they are the most perfect of his experiments, and were made under the inspection of Mr. De Luc. It is a most singular apparatus, to measure the heat of such a light body as air; I can compare it to nothing but weighing a feather in a pair of scales that would weigh St. Paul's.

### EXPERIMENT XI.

" Air in the vault	—	60.7
" One pound, three ounces and two drams		
" of spermaceti oil being introduced into each		
" of the tinned vessels, the temperature in the		
" vessel R, was 59.9; that in L, 60 plus.		
" The cylinders, containing atmospherical		
" air, were heated in an adjoining vault by		
" means of the water bath, and were suddenly		
" immersed in the oil, upon which the central		
" heats being accurately marked during the		
" space of 10 minutes,		
Minutes.		
In 2 R was 60.2	L 60.2	
3 60.9	61.	
4 61.5	61.5 plus.	
5 62. minus.	62.1 minus.	
6 62.3 plus.	62.4 plus.	
7 62.6 minus.	62.7	
	8	62.8

8	62.8	62.9
9	62.9	63.
10	63. plus.	63.1 plus.

" Air in the vault 61.7."

In the first two minutes, the vessel R gained one tenth of a degree more than the vessel L ; in the next minute, the vessel L gained two tenths of a degree more than the vessel R ; in the next minute, the vessel L gained one degree more than the vessel R. Upon the 9th minute, the vessel L gave a sudden spring, and gained two tenths of a degree ; but, upon the tenth minute, the vessel R likewise exerted itself and came up with it. There the race ended ; as it was the point the Dr. wished for, he very judiciously leaving off, in all his experiments, at the point he wants. The three experiments quoted, continues ten, six, and eight minutes, just as they answer his doctrine ; they being such uncertain races, there is no confining them to a certain distance. It is truly ridiculous.

#### EXPERIMENT XII.

- " Air in the vault — 62.6
- " Oil in the tinned vessel R — 62.1
- " In the vessel L — 62.2
- " Air phlogisticated by hepar sulphuris was introduced into the cylinder R, and atmospheric air into L. The cavities of the stems of the cylinders were accurately closed
- " by

" by corks in the manner described above; the cylinders were then heated as before, and immersed in the oil; and the central heats being observed during six minutes.

Minute.

In 1	R was	62.2	L	62.3
2		62.5		62.6 p.
3		63.1 p.		63.3
4		63.7 p.		63.9 p.
5		64.2 p.		64.3 p.
6		64.5		64.6

" Air in the vault at the conclusion of the experiment 63.3.

" This experiment was repeated with the following result."

We shall find this *course* a little more equal, though it ought not to have been so, as they were different airs employed in the experiments: In the first minute, they each gained alike; the second minute, the same; in the third minute, the atmospherical air imparted a tenth of a degree more; in the fifth minute, it lost it again; in the sixth, when the experiment ended, they had both imparted the same heat to the oil, which agreeable to the Doctor's theory ought not to have been.

But these experiments are inaccurate in every respect. In the eleventh experiment, one part of the atmospherical air imparted three degrees, and the other two degrees of heat to the oil in two minutes; but in the twelfth experiment, it imparted four degrees of heat to the oil in two minutes, though the experiment

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periment was every way the same that could affect the thermometer; and so through the whole minutes, comparing one experiment with the other, there are the same differences in all the experiments. Can any confidence be placed in such experiments, in which there is so great irregularity? First one cylinder, and then another, imparting greater heats; this shews that such a small degree of heat is not to be depended on.

### EXPERIMENT XIII.

" Air in the vault — 63.  
 " Oil in the vessel R — 62.3 m.  
 " In L — 62.3 m.  
 " The cylinders, containing phlogisticated  
 " and atmospherical air as before, were heated  
 " and immersed in the oil, and the central  
 " heats being observed during eight minutes,

	R	L
In 1 Minute	62.4	62.4
2	62.7 p.	62.7 p.
3	63.4	63.5
4	64. p.	64.1
5	64.5 m.	64.5
6	64.8 p.	64.9
7	65.1	65.1 p.
8	65.3 p.	65.3

" Two measures of the phlogisticated air,  
 " used in these experiments, having been mix-  
 " ed, previously to its introduction into the  
 " cylinder

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" cylinder, with one of nitrous air, the measures of the test were 2.9. After the experiments were finished, a part of it was drawn from the cylinder, and being again examined by nitrous air, the measures of the test were found to be 2.8, very nearly. From which it appears, that very little alteration had been produced in it by exposure to heat in those experiments."

In the first and second minutes, they imparted the same heat; in the third, the atmospherical air imparted the tenth of a degree more; in the fifth minute, the phlogisticated air imparted a tenth of a degree more; in the sixth, the atmospherical air imparted a tenth of a degree more; in the seventh, the phlogisticated air imparted a tenth of a degree more; in the eighth minute, they imparted the same.

These experiments were the last, the others being full of errors, which he acknowledges and corrected, otherwise I should have given them all regularly as they occurred. The only observation I can make, is, that I am surprised that they could be brought as evidence for the hypotheses, such an apparatus to prove so delicate an experiment; the results were, as we might expect, very uncertain and irregular.

The Doctor seems to be sensible of this, for he says, " It may perhaps be doubted, whether such minute differences may not be liable to much uncertainty." We agree with him,

him, that the tenth of a degree is too minute for observation; but were it ever so plain, it would not in the least avail him, for there is the greatest irregularity in the whole.

Indeed we should have supposed, that there would have been some difference in pure and impure airs, and likewise, that inflammable air would have imparted the greatest heat, according to its specific gravity; from this cause, their having water in their composition. Dr. Priestley, in forming inflammable air, found that water in a great quantity was imbibed into its composition. I have also found, that when rarified by heat, it will absorb moisture in the act of rarefaction, and give it back again when condensed: That this is the case with atmospherical air, has been long ago ascertained by experiments in the air pump.

Moreover, Mr. Darwen found (see Philosophical Transactions, vol. 78.) that air when rarified absorbs heat, and when condensed, gives it back again: Therefore, under this review of the circumstances, we may reasonably suppose, that had there been a perfect apparatus for the experiments, some more heat would have been imparted by the pure than impure airs; particularly, as moisture is imparted by their condensation. This circumstance will, no doubt, strike Dr. Crawford, and he will see, that the great degree of heat produced by his first experiments, and by which he was led to adopt wrong conclusions, was heat imparted

by the moisture to the iron filings. But (not to make use of such an awkward apparatus, which may be properly compared to the weighing of St. Paul's and a feather) take dephlogisticated air in one bladder, and phlogisticated air in another, their temperature of heat being different; and see, if upon their being mixed together, the one imparts so much greater heat than the other. This is making the experiment upon the same principle, as that of the well known one, of the two fluids, *mercury* and *water* when mixed together; which shew such a different capacity for heat, the latter retaining so much more heat than the former, though its specific gravity is so much less: For this same experiment gave rise to this great doctrine of bodies having different capacities for heat.

The explanation of which hath been very happily accomplished by Dr. Harrington. He proves, (see his thoughts on air) that inflammable air, or phlogiston, is concentrated fire, and that when bodies are saturated with this inflammable air or phlogiston, they will imbibe a less quantity of actual fire, or fire in a free disengaged state; consequently, a less degree of heat will raise them to a certain temperature, than bodies which do not possess so much fixed heat or phlogiston; therefore, they will impart less to colder bodies.

We would by no means contradict Dr. Crawford's supposition, that the capacities of bodies for actual heat are very different; and also,

also, that the calces of metals retain a greater quantity of this kind of loose attracted heat, than the metals themselves; that the more of concentrated and fixed heat in the state of phlogiston the calces attract, the greater will be their capacity to repel the more loose and disengaged heat. This doctrine appears reasonable, and is the same in other chemical phenomena. Metallic bodies have their full saturation of fire; so have also the neutral salts a full saturation of acids. But, in one case, if you apply higher concentrated fire to the one, you will expel the lesser concentrated fire in the other. In the same manner, if you apply a higher concentrated acid to the neutral salt, you will expel the weaker; just as the mineral acids expel the vegetable, and so on. (See Dr. Harrington, p. 140.)

As to the other arguments brought by Dr. Crawford in defence of his doctrine, they have been long ago produced by Dr. Harrington in favour of his, and are much more agreeable to, and more strongly corroborate his simple and elegant theory of animal heat; therefore we shall not take notice of them here, but only observe in general, that we have been as concise as possible (yet not to darken, but render the subject perspicuous) throughout the whole of this treatise.

Dr. Crawford's apparatus seems to have been, in every respect, inadequate to the experiments; for it was not impervious to air, since the common air of the atmosphere, he

found after the operation had entered into the cylinders. If so, water must likewise have entered, as the one is more penetrating than the other.

Dr. Black's doctrine of latent heat, was what Dr. Crawford founded his theory upon; but by endeavouring to suit it to his theory, he has distorted it in the manner we have shewn. But this is nothing to what Mr. Lavoisier has done. Dr. Crawford paid some little regard to the principles of it, and to the doctrine of the different capacities of different bodies for heat. But Mr. Lavoisier has with gigantic strides levelled all before him; all must give way to his superior theory. But let us attend to their experiments. In the Philosophical Transactions, vol. 74. p. 348. Mr. Watt says, " This " experiment may be made more completely " by means of the excellent apparatus which " Mess. Lavoisier and De la Place have con- " trived for similar purposes.

" Until direct experiments are made, we " may conclude, from those which have been " made by the gentlemen just named, on the " decompositions of air by burning phos- " phorus and charcoal, that the heat extricated " during the combustion of inflammable " and dephlogisticated air is much greater " than it appears to be; for they found that " one Paris ounce (= 576 Parisian grains) " of dephlogisticated air, when decomposed " by burning phosphorus, melted 68,634 " ounces of ice; and as, according to another " of

" of their experiments, ice, upon being melted,  
" absorbs  $135^{\circ}$  of heat, by Fahrenheit's scale,  
" each ounce of air gave out  $68,634 \times 135^{\circ}$   
" =  $9265^{\circ},590$ ; that is to say, a quantity  
" of heat which would have heated an ounce  
" of water, or any other matter which has the  
" same capacity for receiving heat as water  
" has, from  $32^{\circ}$  to  $9265^{\circ},590$ : a surprising  
" quantity! (It is to be understood, that all  
" the latent heats mentioned herein are com-  
" pared with the capacity of water). And  
" when an ounce of dephlogisticated air was  
" changed into fixed air, by burning char-  
" coal, or by the breathing of animals, it melt-  
" ed 29,547 ounces of ice; consequently we  
" have  $29,547 \times 135^{\circ} = 3988^{\circ},845$ . the  
" quantity of heat which an ounce of dephlo-  
" gisticated air loses when it is changed into  
" fixed air. By the heat extricated during  
" the detonation of one ounce of nitre with  
" one ounce of sulphur, 32 ounces of ice were  
" melted; and, by the experiment I have  
" mentioned of Dr. Priestley's (6), it appears  
" that nitre can produce one half of its weight  
" of deplogisticated air."

Here it appears from their own experiments, what an amazing quantity of heat is produced from an ounce of nitre and sulphur; a quantity sufficient to melt 32 ounces of ice: Then, agreeable to all their doctrines, this great degree of heat must come from the oxygen gas in the nitrous acid. Let us see then how much it contains according to these great chemists,

mists, Mr. Lavoisier, De la Place, and others. They say, "Mr. de la Place and I deflagrated " a convenient quantity of nitre and charcoal " in an ice apparatus, and found that twelve " pounds of ice were melted by the deflagra- " tion of one pound of nitre. We shall see, " in the sequel, that one pound of nitre is " composed, as under, of

Potash 7 oz. 6 gros 51.84 grs. = 4515.84 grs.  
Dryacid 8 1 21.16 = 4700.16

The above quantity of dry acid is composed of

Oxygen 6 oz. 3 gros 66.34 grs. = 3738.34 grs.  
Azote 1 5 25.82 = 961.82."

But they make no allowance for the water that nitre is supposed to contain: Here then, oxygen gas that is contained in this ounce of nitre is not half an ounce; being not double the quantity that the phosphorus consumed, therefore it ought not to have melted 32 ounces of ice. Besides, if the experiments had been made with nitre and phosphorus, the heat would have been considerably greater, as phosphorus is more inflammable than sulphur; nay, I have found, that if phosphorus is well mixed with the purest nitre, a far less quantity of the nitre than one ounce, will do for the combustion of one ounce of phosphorus: Besides, in forming nitre into dephlogisticated air, a great part of the nitrous acid goes over entire into the receiver; therefore, it could not all go to the formation of the dephlogisticated air.

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But Dr. Harrington hath sufficiently explained how the dephlogisticated air in this process is produced.

" It is very extraordinary in mysterious subjects, what imperfect reasonings will be dispensed with. The water of crystallization in the nitre, is estimated at 34 grains, 487 grains of air is produced; and yet it is supposed that this air is produced by the decomposition of the water.

" Let us take a general view of these important experiments. Dr. Priestley got from 2 ounces, or 960 grains of nitre, 487 grains of air; the nitre lost in the experiment, 531 grains. Mr. Kirwan computes that 960 grs. of nitre contain about 326 of the nitrous acid, and 115 of water. Hence there is more air generated than the weight both of the acid and water; then we must take in the alkali as making a part of the air, according to their nice calculations. But when we consider that in the distillation of nitre, there is a considerable quantity of the nitrous acid that goes over entire into the receiver, more of the alkali must in consequence have gone to the formation of the air. The loss in the nitre was 531 grains, and as none of the alkali is supposed to go over entire into the receiver, and as the nitrous acid does, it must therefore have added more considerably to the formation of the air. In the process, whether it yields pure or impure air, it matters not, as it is allowed

“ allowed that the impure air is formed of  
“ the nitrous acid and phlogiston; and I al-  
“ low that it is the same air as dephlogisticat-  
“ ed air, but only having a weaker concen-  
“ tration of fire.”

Dr. Priestley has likewise clearly proved, that in air being formed from the nitrous acid and mercury, there is a considerable loss of the mercury. He says in his preface to vol. III.  
“ Being unwilling, however, to depend wholly  
“ upon my own address in experiments of this  
“ kind, I consulted Mr. Magellan (who, in-  
“ deed, first suggested to me his suspicion that  
“ the Abbe must be mistaken in the fact) and  
“ he engaged Mr. Wench jun. (whose skill  
“ and care in chemical processes, no person  
“ who is acquainted with him can question)  
“ to make the experiment in the most accurate  
“ manner he could devise.

“ Accordingly he dissolved an ounce (Apo-  
“ thecaries weight) of the purest mercury, in  
“ the purest nitrous acid; and both myself  
“ and Mr. Magellan were present, when he  
“ revivified exactly one half of the red precipi-  
“ tate made from it in a glass retort, surround-  
“ ed with live coals, in a *reverberatory* furnace;  
“ when the mercury has wholly sublimed in-  
“ to the neck of the retort, a very small quan-  
“ tity only of a brownish matter remaining  
“ unsublimed. The whole being carefully  
“ weighed, together with the retort, which  
“ had also been weighed before the process, it  
“ appeared there was a loss of 88 grains, which  
“ is

" is something more than one third, of the weight of the quicksilver. We are all satisfied that it was not possible to make the experiment with more fairness."

' Another comment we must make upon the experiments of Dr. Priestley. Two ounces of nitre in one process, yield 787 ounce measures of impure air, viz. 1.25. The same body in the same process, yields 812 measures of pure air, viz. 0.95. Now phlogisticated air and dephlogisticated air, are supposed to be two of the elements or bodies which compose the nitrous acid, and here is more of these airs produced than the weight of the nitrous acid. But which is more striking than this, the nitrous acid produces in one experiment, principally one of its elements; and upon repeating the process, another of its elements is produced, and to such a considerable quantity, one being above the weight of the acid lost, allowing for what comes over entire. Then, agreeable to their theory, the alkaline salt decomposes the nitrous acid in the one process, by attracting its pure air, and in the other its impure air; can we possibly reconcile such contradictions? And this impure air being more considerable than they will allow the acid to contain.

' Whether my theory is just, I leave to the impartial world; but I must assert those theories to be erroneous. My theory immediately accounts for these phenomena; the nitrous acid unites with the alkali and water,

and with the fire they are all neutralized and aerilized. But agreeable to the quantity and manner of the fire's being employed in this process, the air will have different im-pregnations.

The acids generate air as we might have supposed, a priori. The mineral acids do it the best, as having the strongest attraction for the earths and alkaline salts; therefore they are aerialized with the highest saturation of these three bodies, viz. acid, fire, water and earth, or salt. The nitrous acid is the best of these, as having the greatest attraction (allowed since the first days of chemistry) for phlogiston or concentrated fire. The metallic earths are better than other earths, for the same reason, having a strong attraction for concentrating fire; proved from their being capable of being reduced by fire alone, and still more particularly, the calx of lead and mercury are the best, as having the greatest attraction for phlogiston, being easily reduced, even with fire alone. But the vegetable acid being so weak, having so much less attraction for the earths, salts, and concentrated fire, it therefore makes an imperfect or impure air.'

Now let us consider more particularly the circumstances, half an ounce of oxygen gas, in being condensed into the phosphoric acid, is supposed (agreeable to their theory) in its condensation, to produce heat equal to melt 34 ounces of ice: But when a less quantity of

of oxygen gas is already condensed, and hath given out the heat, which it does in its condensation, that it should produce nearly an equal quantity of heat, so as to melt 32 ounces of ice, is an evident absurdity. Besides, as we have before observed, if this same quantity of nitre was applied to phosphorus, it would have melted more than 34 ounces of ice. Oxygen gas in its aerial state, is not in the least acid, while it is supposed to possess its caloric; but it loses it upon being imbibed into the sulphur, and phosphorus forming the vitriolic acid, and phosphoric acid. Then, when it has entered into the nitrous acid, forming that strong mineral acid, the first in point of acidity, at least, next to the vitriolic acid, which they suppose contains no caloric. Can we seriously think it takes with it all its caloric? Nay, it must positively take more with it than the oxygen gas possesses, as it produces in combustion with phlogistic bodies, more heat than could be produced from the same quantity of oxygen gas, which they say it possesses. But if we are to philosophize upon this experiment, agreeable to Dr. Black's and Crawford's theories, the wonder will be still greater; for in the experiment, the sulphur of burning nitre and sulphur, is formed into the vitriolic acid, and likewise different aerial bodies are formed; so much as, had the authors of this extraordinary theory, endeavoured to prove that a degree of heat, equal to the melting of 32 ounces of ice, had disappeared in-

stead of appeared, this experiment would have made them ride triumphant. But it is too absurd for men who deservedly rank so high in chemistry to defend. For heaven's sake listen to the voice of reason, nothing can be gained by such a controversy \*.

But let us see, gentlemen, if combustion will aid your theory in other processes. The strong concentrated nitrous acid, being mixed with high phlogistic bodies; such as essential oils, inflammable air, &c. will produce violent ignition. But in these experiments they suppose

\* Which train of reasoning upon this difficult subject is the more logical? There undoubtedly are many cases in which the temperature is lowered as a body dilates, and where an apparent influx of heat produces no effect but expansion; and again, where an increase of temperature accompanies contraction of bulk. Then what can be more plausible than the idea conveyed by capacity for heat, and the analogy of water alternately imbibed by a sponge, and squeezed out of it, by which we gain at least some conception of a mechanism placed so far beyond the sphere of the senses? It has always been a recommendation of false opinions when they were accompanied by some analogy easily grasped at by the imagination.

I am very doubtful how far the specious theory of our philosophers will, this time, prove to be the system of Nature: does not the very familiar experiment of the deflagration of gunpowder present appearances directly contrary to its principles?—not to enumerate the other well-known instances of mixture, where much heat is generated, and at the same time, an abundant extrication of aeriform matter is observed—here we have great expansion and violent heat. Will it be said, that both the heat, that goes to constitute elastic fluids, and that becomes sensible, flows out from the mixed materials in consequence of a diminution of their capacity; and was already contained in them? Can this be proved in the case of nitrous acid and oil, black wadd and oil, lamp black and oil, iron filings and sulphur?—Dr. Beddoe's Chemical Experiments and Opinions, p. 12.

suppose, that the oxygen gas of the nitrous acid, leaves its nitrous base, and unites to the inflammable air, or the essential oils. Then, agreeable to Mr. Lavoisier's theory, the oxygen gas uniting to the *carbone* of the essential oils would form fixed air, and this fixed air should be equal in weight to near the whole of the nitrous acid employed. This is agreeable to their theories, but we find no such quantity of fixed air formed ; nay, in some experiments, not even an atom of it. That acids may be aerialized into different acid airs, either nitrous or fixed, is agreeable to Dr. Harrington's theory, and what we might have supposed a priori.

Gentlemen, I will not dispute those extraordinary chemical attractions, but only observe, that in all your theories, which are a mass of strange contradictions and absurdities, you make the table of chemical attractions run the gauntlet. In this experiment of the nitrous acid and oils, there is a great heat generated, and likewise an abundant extrication of aeriform matter. Now, let us see if we cannot investigate the cause : the oxygenated muriatic acid will equally produce combustion in these experiments, but the common marine acid will not ; the difference in these two acids, is neither more nor less than this ; the one is phlogisticated, and the other deplogisticated. Mr. Lavoisier says, That the one possesses more of the oxygen gas than the other ; this is not so, for the process by which  
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the oxygen marine acid is formed from the common marine acid is, by adding *minium* or *manganese* to the latter, which are supposed to impart their oxygen gas to the acid. Dr. Harrington has given a rational and just explanation how the lead acts in dephlogisticating the marine acid. This effect is produced from the acids and earth, which, when united, have a superior attraction for phlogiston, than when separated.

' In forming pyrophori, so as to retain a quantity of fire necessary to make them, there is required an acid and an earth; for Mr. Bewly found, when he took all the acid from the earth, that it would not make a pyrophorus. One of the most striking facts, which shews how necessary it is to have an acid and an earth in order to attract fire or phlogiston, is the experiment upon the nitrous vapour, which being so condensable, as a colder body, is capable of doing it, and attracts its fire from it, and condenses the acid; but if it pass through water it attracts a quantity of the water sufficient to make it become a permanent vapour, or the nitrous air. Chemistry affords us a number of facts. Manganese or the calces of lead will dephlogisticate the marine acid, so as to give it the power of dissolving gold. Both the manganese and the earth of the lead is saturated with the acid, and together they have the power of attracting the phlogiston from the rest of the acid. So that when this dephlogisticated

gisticated acid is exposed to gold it will dissolve it from its increased appetite for the gold's phlogiston.

If into a solution of iron you put more iron it will be dissolved, and crystals will be formed. The calx in the solution being saturated with the acid; upon fresh iron being added to them, they attract the iron's phlogiston, and crystallize. Mr. Kirwan in the Philos. Trans. vol. lxxiii. p. 74. says, "This increased affinity of the calx of iron to phlogiston is not a mere supposition; for, if into a solution of iron, so far dephlogisticated as to refuse to crystallize, some fresh iron be put, the impoverished calx will re-attract so much of the phlogiston given out during the solution of the fresh iron, that it will now afford crystals, as Mr. Monnet has observed in his excellent Treatise on Vitriolization."

The very singular fact is owing to this, that copper is capable of decompounding a solution of iron, and iron of decompounding a solution of copper. The calces of each, when united with the acid, have an increased appetite for the phlogiston of each other's metal. There are many other facts in chemistry that we may bring to establish this fact.

But, according even to Mr. Lavoifier's theory, the oxygen gas which the calces of the lead can impart to the marine acid, must have lost all its caloric; for in the combustion of metals, a great heat is produced:

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This, he says, is from the oxygen gas imbibed in the process: Nevertheless, even this oxygen muriatic acid, acts with more violence in combustion, than the nitrous acid. Moreover, Dr. Harrington says, That the oxygenated muriatic acid is not made from the calx of lead imparting to it its oxygen gas; because, though the calx of lead has had all its oxygen gas, previously taken from it by fire, it will nevertheless oxygenate the marine acid. But that this acid (which Dr. Priestley, with great propriety, calls dephlogisticated) is formed by being dephlogisticated by the calx of lead, is, I think clear; for, if phlogiston, in any form, is added to it, it will become the common marine acid.

But even suppose we were to allow them these strange suppositions, that combustion is sometimes owing to the oxygen gas in the marine acid, imparted by the calx of lead: The combustion is more considerable than can be accounted for from so small a quantity of oxygen gas as the calx of lead could possess, or impart to the acid; but with our explanation is perfectly easy: For, the dephlogisticated marine acid attracting phlogiston so strongly, rushes to an union with such violence, as to set the dormant fire loose; and such a quantity being set at liberty, will set fire to the rest of the composition.

But let us attend to Dr. Harrington's explanation of these phenomena. The nitrous and dephlogisticated marine acids, having the greatest

est attraction for phlogiston, (an old and well ascertained chemical fact) will, when mixed with high concentrated phlogistic bodies, (which phlogistic bodies are formed of fixed fire) forcibly and chemically attract this fire; and this fire in these phlogistic bodies is attracted by the acid of fixed air, or other bodies, which have a less attraction for it. Consequently, such a quantity of it will be set loose as is able to produce ignition. We have, in this essay, explained the principles of these facts.

That this explanation is just, appears very probable from the vitriolic or other acids, which being added to the essential oils, great heat will be produced, but not enough to produce ignition, because they possess not so great an attraction for concentrated fire or phlogiston. Again, if the nitrous and marine acids are phlogisticated, or not so high concentrated, they will produce heat, but not enough to produce ignition.

There is an experiment which very strongly shows the principle of combustion. If the least moisture falls upon a body formed of the nitrous acid, the caloric from lime, and the volatile alkali; or even some times if it but suffers agitation, it will forcibly explode. By moisture it makes the heat become more actual, (the effect being the same as it has upon quick lime) and aided with the power of the nitrous acid, they both penetrate together into

the volatile alkali, and decompose its phlogiston into actual fire.

But let us attend to combustion in other processes. The nitrous vapour, which is obtained by inflammable air, passing through nitrous acid, attracts part of the acid and keeps it suspended. Here then is no heat to keep it in an aerial form; for, upon its being again condensed by water, no heat is received from it: But if these two bodies are fired, there will be a great explosion, with an immense quantity of heat. Dr. Crawford is forced to acknowledge, that this nitrous vapour possesses as much caloric as dephlogisticated air. And, Dr. Priestley, in accounting for this phenomenon, supposed that this nitrous vapour, from its producing such violent ignition, must be dephlogisticated nitrous vapour; but, as Dr. Harrington says, if this vapour is nicely examined, it will be found to be the true nitrous acid undecomposed.\*

Mr. Lavoisier's hypothesis supposes that the acid vapour is decomposed by the inflammable air; that is, the inflammable air attracts the phlogisticated or nitrous air, and leaves the dephlogisticated air free to explode with the remainder of the inflammable air. Those great

\* It is probable that the vapour of pure nitrous acid, contains as much absolute heat as atmospherical air; for the power of the former, in maintaining flame, is nearly as great as that of the latter. Hence we may account for the spontaneous accension of a mixture of oil of turpentine and nitrous acid.—Crawford's Experiments and Observations on Animal Heat, p. 420.

great theorists differ widely upon so simple an experiment. Inflammable air, by passing through the nitrous acid, attracts part of the acid. That this is simply the case is demonstrable; for if, (as Dr. Harrington observes) previous to the operation of its being burnt, if the inflammable air, along with its acid vapour, is passed through water, the acid will be imbibed by the water, and the air will lose the power of burning; and if the water is examined, it will be found to have imbibed the pure nitrous vapour, or acid. But Dr. Priestley, agreeable to his hypothesis, thinks it is dephlogisticated nitrous vapour: However, if the acid is examined when it is received, by the water washing it from the inflammable air, it will be found to be rather phlogisticated, having imbibed a little phlogiston from the inflammable air.

It is surprising how vague and undetermined all these great theorists are upon every experiment and phenomenon. They always have different opinions, each being in the dark, and each following their vague conjectures.

It is certain, that not the oxygen gas, but the pure nitrous acid, has to do in this process; for after the combustion, the acid is found entire. Likewise Dr. Priestley found, that if the acid vapour and inflammable air were not fired immediately, but stood long enough, so as to form an union, they would not burn of themselves; but upon being set on fire with atmospherical air, they burnt like

a mixture of nitrous and inflammable airs. See Priestley, vol. III. p. 261. However, it ought to have been the reverse. For while the acid remained entire, it ought not to have burned, but upon its being decompounded, so as to form nitrous air, it must have impregnated them with its oxygen gas, and so have been more adapted to ignition.

But when we examine it, according to Dr. Harrington's theory, we find an easy and rational explanation. For, as the acid vapour has a strong attraction for concentrated fire or phlogiston of the inflammable air, so when fire is added to them, the fire and the acid penetrate together into the fixed fire and set it free, producing that violent explosion. But if they act not together, no ignition is produced; because, if the acid is exposed for one day, or any certain time, so as that its acidity may be neutralized with phlogiston, the consequence is, it loses its attraction for phlogiston or fixed fire. The whole of which Dr. Harrington has set in the clearest point of view, and proved to a demonstration.

This will appear in a still more striking light in alkaline air, when formed by heat into inflammable air. No chemist, I presume, can be a stranger to the strong attraction which alkalies and acids have to each other; therefore, if in the experiment, the alkaline air is made use of, our explanation will appear the more evident. Nature has wisely ordained that no ignition can be produced but by the operation

operation of two causes at the sametime. A combustible body, though fire is applied to it, will not burn without the agency of another body. This great agent in nature is atmospherical air; and in chemical processes, either nitrous acid, or dephlogisticated marine acid. How they act has been already explained; and this explanation will greatly assist us in showing how atmospherical air acts. Atmospherical air, or the pure part of it, is a compound of an acid and phlogiston: This we shall undisputably prove afterwards, and find that this composition is capable to produce fire. But atmospherical air being composed of a mild acid, and of a mild not of a high concentration of fixed fire, parts with its own fixed fire, when actual fire is applied to it; however, the heat it produces is not powerful enough to keep the air burning, or decomounding its own fixed fire, unless it is exposed to some combustible body having a high concentration of fire.

Again, if fire is applied to a combustible body, not having the aid of this compound; which is composed of an acid and a weak concentration of fire, called atmospherical fire, it will not burn; its fixed fire being so closely and strongly attracted, as renders it impossible to be decomounded. This theory is very strikingly confirmed in what chemists call dephlogisticated nitrous air. If this air is formed not of a strong saturation of phlogiston, it will allow a candle to burn in it with a bright and vivid flame; but if it has gotten a higher saturation

tion, it will burn with greater vehemence, and the flame will extend over a greater part of the air; that is, the flame will become extended from the candle, and more of the air will be burning at the sametime: And if saturated with a still greater quantity of phlogiston, it will all take fire together, and burn with an explosion like to that of inflammable and atmospherical air; the agent and the principle being both in the same body.

To these facts I would have the reader pay a particular attention, since by doing so, he will be the better able to understand the principles of bodies burning. For I pledge myself to prove in the clearest manner, and I hope to his satisfaction, that this dephlogisticated nitrous air is formed of the nitrous acid and phlogiston; so that, if he is not bit with the tarantula of modern chemistry, it is impossible he should mistake it.

Air is a combustible body as well as the body which it consumes. Nature's own air being only a weak saturation of fixed fire, forms only a weak combustion; therefore, bodies burning in it, burn comparatively very gently, consuming themselves in a slow and gradual manner. And nature has happily joined its own air with another kind of air, which is not combustible; so that a small surface of the phlogistic particles shall come in contact with the flame, or burning bodies. But artificial dephlogisticated air having a higher saturation of fixed fire, or phlogiston, allows

lows a candle to burn with a more extended flame, and with cracklings, or (as Dr. Harrington calls them) partial explosions. And if a still higher saturation, (being before an agent to produce ignition in another body) it will in this case become both agent and principle, and decompose itself. For Dr. Priestley formed this air with so high a saturation, as to burn and explode like inflammable air \*; and, upon a particle of fire being added to it, it immediately consumed itself. For, containing the two principles of combustion, viz. the agent, or nitrous acid, and the principle, which is a high saturation of phlogiston, from either essential oils, metals, spirits of wine, &c. all of which will form it; this effect necessarily followed: But had the principle been less, highly saturated, it would not have consumed itself; because, one of nature's invariable laws is, that the action of two bodies is required to support fire; otherwise, were the bowels of the earth set on fire, the fire would not stop till the whole were consumed; and the same would happen with regard to air.

The aerial forms have been so mysterious to chemists, that to give a rational and philosophical account of them, has perplexed them much; yet we can bring solid bodies to confirm our theory. For the nitrous acid, or the dephlogisticated marine acid, when mixed with

\* Viz. dephlogisticated nitrous inflammable air.—See Priestley, vol. IV. p. 455.

essential oils, or the volatile alkali, will just equally promote ignition. A further corroboration of this theory is, if pure, strong, actual fire, (as in the electrical spark or fire) is applied to atmospherical air; it will decompose it in a slow and gradual manner; only repeated application is required. And, that this electrical fire acts as pure fire, is what we shall prove in the sequel.

- We have endeavoured to prove, that atmospherical air is formed of an acid, with a weak saturation of phlogiston or fixed fire, that therefore it will not burn or decompose itself, without the aid of a body containing a higher saturation of fire. This is evident in salt-petre, which being an acid united to a weak saturation of fixed fire, acts in combustion very similar to atmospherical air. Therefore, it will not burn of itself, unless united to a body of a higher saturation of fire; and then both together will produce fire with great vehemence. But take the nitrous acid, and unite it to an exactly similar body, only of a higher saturation of fire, viz. the volatile alkali, and they will burn of themselves without the aid of another body. This is just the same as highly saturated dephlogisticated nitrous air burning without the aid of a third body. Nay, by only aerilizing this nitrous flammans, it will form this identical air, viz. dephlogisticated nitrous air, though not altogether so inflammable, from being not so much condensed.

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It is strange to think what singular theories have been adopted by our modern chemists; when this same nitrous ammoniac or *nitrum flammans* was aerilized, which is easily done, being a volatile body, they would, from its forming the nitrous dephlogisticated air, have the acid decomposed. The volatile alkali (our fathers in chemistry have consequently misnamed it) remains fixed, decompounding the acid. The explanation which we have given, being simple and plain, cannot possibly be mistaken, as they are both volatilized. For, in the name of wonder and common sense, do they not equally burn in their solid state, as in their aerial? Are not our senses convinced that they are united in this solid state, without having recourse to the aid of *the aerial philosophy*? But according to this theory of theirs, they leap over the most obvious facts.

It is natural for the human mind, when travelling in the dark, to fix upon something, no matter how preposterously absurd; but when a principle is laid down which fully explains all the phenomena, not to embrace it, is certainly highly wrong.

The dephlogisticated marine acid air has a considerable degree of attraction for concentrated fire; and, when the volatile alkali is added to it, it burns very beautifully, forming (allow the expression) a sea of fire.—As our doctrine of ignition depends upon fixed fire being set loose, and as we have proved that the nitrous acid will do it of itself, when unit-

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ed to high phlogistic bodies, such as essential oils. Nay, even bodies with less phlogiston, will be set on fire without heat, when an acid of a stronger attraction is employed; viz. the neutral salt, formed of the dephlogisticated marine acid and fixed alkali: It will fulminate with the least friction, as Mr. Sage found. Yet, when the alkali is united to the vitriolic acid, it will not fulminate, even when mixed with combustible bodies: But if the nitrous acid is added to the alkali, being an acid of a stronger attraction for fire, it will then fulminate, when mixed with combustible bodies, as in gunpowder, &c. yet, after the combustion of the marine neutral salt, if the residuum is examined, we will find the marine acid entire, and not decompounded.

A very striking experiment is this; expose many different metals to the dephlogisticated marine acid air; viz. iron, copper, lead, and many others, and ignition will immediately take place, without any heat being added at the time. And let Mr. Lavoisier and his advocates well examine the calces of the metals, after the combustion, and they will find them composed of the earths of the metals and the marine acid, and not of the oxygen gas: Because the marine acid air decompounds the phlogiston, and sets the fire loose.

There is another chemical process that can give us great assistance in this investigation. Lemery found, that if sulphur, iron and water are made into a paste, they would take fire in

so striking a manner, as induced him to suppose that these three bodies are the causes of the violent concussions, which sometimes happen to our earth. Sulphur is formed of the vitriolic acid and concentrated fire; iron of concentrated fire and earth of iron. Now, there is a strong attraction between the vitriolic acid and the earth of iron; but the concentrated fire hinders their union. Moreover, the water which has likewise a strong attraction for the earth of iron, (seen in a most striking manner in the steam of water having a power of itself to decompose iron without the aid of the sulphur) will, along with the sulphur, decompose the iron. The motion or fermentation produced by these new compositions and decompositions, will set the fire loose, and produce great expansion and flame. The phlogiston of the sulphur and the iron have escaped after the combustion; and nothing but fire did escape: Therefore the phlogiston must have escaped as fire; for the residuum is the vitriolic acid united to the earth of the iron with the water.

Let us see if their theory will give any satisfactory solution of these phenomena. The vitriolic acid is formed and the iron reduced. Air could not do it; for the combustion will take place (as Lemery found) where air cannot have access. Then, if we allow them their doctrine, it would (I will take upon me to say) be giving countenance to the most extravagant

and extraordinary doctrine ever seriously presented to the public.

For, according to their theory, water is formed of inflammable and dephlogisticated airs. Then, (according to Mr. Lavoisier) upon the sulphur and iron attracting the dephlogisticated air of the water, the inflammable air would be free and set loose in the experiment : But were this the case, we should not only have found, after the combustion, the phlogiston of the sulphur and iron, but likewise the phlogiston of the water. But they have all disappeared ; and instead of heat being produced, an immense degree of cold should take place, the heat going to the expansion of the inflammable air. If we pay the least regard to Dr. Black's ingenious doctrine of latent heat, we must see that this is agreeable to it ; and it is upon the principle of latent heat that their new doctrines are founded. But even to waive this argument ; we find that the water is not decompounded in the process, for it is found entire, the phlogiston only having *disappeared*, and the heat *appeared*. Hence we find that, by chemical fermentation (which shows itself by great agitation or ebullition) heat is set free, which brings us near to Sir Isaac Newton's theory : But, as Dr. Harrington has shown, it is only the motion of the particles of this particular body called phlogiston.

We have tried the truth or falseness of Mr. Lavoisier's theory by the experiments he has produced to confirm it, and which he thinks most

most favourable to his doctrine. But even in these experiments, he has taken only one view of them; for, upon an accurate investigation, we shall find that even they contradict his theory.

When strong nitrous acid is mixed with essential oils, a vivid flame with active combustion takes place, and with a comparatively small generation of nitrous and phlogisticated airs.

The new theorists, to explain these phenomena, say, that the oxygen gas of the acid unites to the *carbone*, or phlogiston of the essential oils: But then, in this case, an immense quantity of fixed air ought to have been formed; as essential oils, when burned with pure air, form fixed air, and that nearly in weight to the pure air. Now, by attending to their own calculations in p. 14, we should have expected to have found a quantity of fixed air, nearly in weight to the quantity of nitrous acid employed in the experiment. However, the fact is this; if the ignition is vivid and strong, not an atom of fixed air is found after the combustion.

But, (as Dr. Harrington hath clearly demonstrated) agreeable to the intenseness of the fire, the air produced will be of different kinds; because these acid airs are clearly the different acids aerilized, and their particular kind depends upon the process.

This fact is strongly proved by the experiment of burning nitre and charcoal together, which (as Mr. Cavendish says) form only phlogisticat-

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ed air. He also says, (Philosophical Transactions, vol. lxxiv. p. 135.) "Before I enter into the cause of these phenomena, it will be proper to take notice, that phlogisticated air appears to be nothing else than the nitrous acid united to phlogiston; for when nitre is deflagrated with charcoal, the acid is almost entirely converted into this kind of air. As far as I can perceive too, at present, the air into which much the greatest part of the acid is converted, differs in no respect from common air phlogisticated."

Now, here is a very severe blow given to Mr. Lavoisier's doctrine; for it is evident that the oxygen gas of the nitrous acid and the charcoal do not produce fixed but phlogisticated air: Yet this charcoal is his *carbone* and from it his theory of the composition of fixed air has originated. But this doctrine shall undergo a more particular investigation in the sequel.

This theory appears still more defective in accounting for the *caloric*, which, in their processes, is produced by burning the nitrous acid with bodies, which, according to Mr. Lavoisier, contain *carbone*; but phlogiston, according to others.

The difference of heat produced in combustion, when such bodies as charcoal, essential oils, &c. are employed; bodies which reduce oxygen gas to fixed air; and phosphorus, sulphur, &c. which reduce oxygen gas to acids: I say, the difference of heat produced by combustion in forming strong acids, is above twice the

the quantity to what it is when the oxygen gas forms fixed air. Therefore, in burning bodies, principally formed of carbone with acids; instead of caloric, an equal degree of cold ought to have been generated. And also in the combustion of the marine and nitrous acids with essential oils, nitre and charcoal; instead of heat there ought to have been an immense degree of cold produced in aerilizing the fixed air, which these processes should generate.

Nature having wisely ordained, that the atmospherical air, a light aerial phlogistic body, should be the agent, by which combustion is conducted, as being easily susceptible of taking fire. But then its fire is so light and weak, that it is not capable of consuming or burning, without the aid of another combustible body, which is set on fire by the atmospherical fire being kindled; and then, the strong combustible body, (as a candle for instance) is also lighted, both fires acting together, so as to keep up the combustion. But were the fire applied to the candle without the agency of atmospherical air, no combustion would follow, even suppose the candle is composed of inflammable materials. For, both atmospherical air and a combustible body are required to produce ignition, and that upon account of the attraction and concentration of fire. There will be no combustion, unless they both act at the same time; or, that intense heat necessary to support the ignition, or the consuming of the body, will not be produced.

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The cause of which is most probably this; the intense heat produced acts upon the combustible bodies concentrated fire or phlogiston, so as suddenly to expand them, and to break their attraction from the bodies with which they were chemically united. For the consumption of one particle of its fire, is the means of consuming the neighbouring particles, and so on, till the whole body is consumed, or broken down by the separation of its concentrated fire or phlogiston; and from the mechanical expansion, a consequence produced from great heat; so that the whole texture of the body will be broken and reduced to ashes. This is evident from intense heat consuming or burning bodies without ignition; viz. light combustible bodies, as paper, which, being exposed to a great heat, will be equally consumed or burnt, as if it had been ignited. But that it is not ignited is evident from the air not being acted upon by the paper: And the same phenomenon will take place in foul air, or in *vacuo*. Hence it is evident that the paper is consumed by having its phlogiston or fire separated from it by the heat.

From this cause it is, that even attrition can act in the same mechanical way, by dissolving the union of phlogiston, or setting loose the fire which form it, from its chemical union or attraction; and when it is set loose it becomes heat or fire. The mechanical and chemical operation of bodies is easily connected: For instance, take two pieces of wood, and, by the

the aid of great mechanical powers, let them be rubbed the one against the other ; this will produce fire. We shall suppose, that the fire produced by rubbing the two pieces of wood was chemically united in the state of phlogiston to a degree of power equal to the force of fifty pounds weight. Now, if a degree of power equal to sixty pounds weight is applied in the form of attrition, it will in consequence overcome the chemical.

That this really takes place in nature, is clear from the experiments performed by Dr. Darwin ; (see Philosophical Transactions, vol. lxxviii. p. 43.) He has shown by uncontrollable experiments, that the thermometer falls by the sudden expansion of the air, and rises by its compression. Now, this demonstrates, that the chemical attraction may be overcome by mechanical force : For when the air is allowed to expand itself, it receives and chemically attracts a great quantity of heat ; but when compressed, that chemical attraction is counteracted, and the heat becomes sensible again to the thermometer. According to this general rule, I would thus define chemical attraction ; that whenever a body, possessing a quantity of heat, is exposed to the influence of another body, of a lower temperature, it will not impart its heat to that body. This therefore implies, that its heat is not loose or active, but is under the influence of some power which retains it ; which is real and chemical attraction only. As in all the other powers or

agents in nature consists of different influences; therefore some bodies attract their fire with greater force than others. For can chemical philosophers suppose, but that any chemical attraction might be overcome by mechanical powers. For example; the particles of acids and alkalies have a strong chemical attraction; but suppose you could fix a cord to each of these particles, would you not be able to separate them? you certainly would. And if two dry bodies, the particles of which act strongly upon each other, are rubbed together, this will be the case. Hence, it will be found, by taking a general view of attrition as producing fire, that it acts according to the mechanical force exerted, and to the bodies retaining their concentrated fire. It is more loosely concentrated in wood than in metals: But the texture of wood being softer, there will be less force; yet, from its phlogiston or fire being more loose, it is more easily ignited; as is seen in its actual ignition by air and fire: Wood burning with more ease than metals. The great Sir Isaac Newton proved that mechanical force can produce fire; but it is only in bodies that possess phlogiston. To this I believe there is no exception. However, the phenomenon is strikingly seen only in such bodies as will admit of this mechanical force, and which possess a great quantity of phlogiston: For example, in iron and flint, bodies which have the most phlogiston.

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This will reconcile the chemical and mechanical idea of heat, which has been the occasion of so much controversy amongst philosophers. For (as Dr. Harrington well observes) no bodies, but those which are phlogistic will produce fire; phlogiston being concentrated fire. And he says further, (see his Thoughts on Air, p. 209.) I agree with Sir Isaac Newton, that heat consists in motion; but it is only the motion of those peculiar particles of fire or phlogiston. The motion of other bodies will not produce it.

But when the alkali is united to the dephlogisticated marine acid, the aid of no other combustible bodies, nor even heat, but only a little friction is required; the mechanical action of which forces the two bodies to a closer union, and by that means aids their attraction. The acid, though just ready to rush in upon the fixed fire of the alkali, is not able, because of the weakness of its acid, to break the union of the fixed fire; but aided with the friction it accomplishes it.

Dr. Harrington, whose theory in all its different principles has been fully proved, calls alkaline salts phlogistic bodies; but says, that they are composed of phlogiston, with a less concentration of fire than oils, bitumens, sulphur, &c. This is an experiment which fully justifies and confirms this opinion, however much it may have been ridiculed. The marine neutral salt, from its fulminating power, is an important body in the investigation of

combustion. It is a body formed neither of the *carbone*, nor of the inflammable air of Mr. Lavoisier, nor of the phlogiston of Dr. Priestley and others; and yet it will burn.

And if we attentively examine the residuum, we shall find that the alkali has disappeared, and the acid is left with moisture. What has separated in the fulmination but heat and light? Need I make the application? It would be to affront the good sense and judgment of my chemical readers.

Do we not observe their extravagant theories contradicted in the investigation of even their own experimental proofs? But will it not surprise my reader, when I tell him, that Dr. Harrington published his complete system in the year 1780? That he kept it many years by him, before he had the resolution to make it public; and, though since that time he has been elucidating and explaining it by different publications, yet the only reward he has received has been the greatest insults and neglect.

Let Mr. Lavoisier and Dr. Crawford try, if they can possibly reconcile their theory of caloric with the above-mentioned experiment: Upon the acid and the alkali uniting, the fixed air, which the alkali possesses, attracts the caloric of the acid and alkali, and assumes an aerial form. This is their own explanation. That all the heat will be expelled is evident, from the acid uniting with the caustic alkali. All the heat then becomes sensible: And, agreeable to Dr. Crawford's theory, as the alkali appears

appears now a body so phlogistic, it will expel the heat from the acid, the same as the volatile alkali would do: And certainly they must acknowledge that to be a phlogistic body. Then, as all the heat was expelled from this marine neutral salt, from whence came the heat which escaped from it, when it fulminated? without doubt from the alkali. The present chemists must be forced to acknowledge the truth, and the examination of the residuum is what will effectually do it. The alkali has disappeared; the marine acid and water are left behind. Can we wish for proofs more clear and obvious? Had not the alkali disappeared; in this case the modern chemists will, I hope, give me leave to assert, that it must have united with the marine acid, and so have still formed a neutral salt in the residuum: Their caloric only having disappeared. I flatter myself therefore, that this remnant of old chemistry is still left, namely, that acids and alkalies attract each other, so as to form neutral salts.

It is evident that alkalies are salts formed of concentrated or fixed fire, and that in order to set that fire loose, bodies which have the strongest attraction for it, viz. the mineral acids, are required. The attraction for fixed fire, when united to the common marine acid, is not strong enough; because, in that state, it is united to some fixed fire or phlogiston. But take that away by the calx of lead, and then, by being robbed as it were of its own phlogiston,

phlogiston, its attraction for it becomes so strong, that it will, of itself, penetrate into fixed fire of the alkali, break its bond of union, and set it on fire all at once with a considerable explosion.

The common nitre is something similar to the factitious dephlogisticated air of chemists: Indeed it is the body which forms the largest quantity of it. But neither the air nor the nitre will burn of themselves, without the addition of a higher phlogistic body, as oil, coals, &c. the reason is, the acid has not strength enough of itself. But when the nitrous acid is united to a higher phlogistic body, as the phlogiston of metals, the oil of turpentine, or spirits of wine, it will then fulminate, fire being set to it, producing Dr. Priestley's inflammable nitrous dephlogisticated air.

The concentration of fire, in atmospherical air, is something similar (as Dr. Harrington observes) to the concentration of it in alkalies, more than in oils, spirits, inflammable air, &c. but not altogether so strong as in alkalies \*. High phlogistic bodies, such as inflammable air, or the volatile alkali, by being first exposed to great heat, and then mixed with the nitrous vapour, will explode. This shews that what the acid wanted in strength, is made

\* In the metallic solutions of metals in acids, when exposed to a long intense heat, metallic earths will form saline deposits. This proves that they become saline, from imbibing the fire. See Priestley, vol. iv. p. 413, 489. These deposits, if chemically examined, will be found to have the characteristics of salts,

made up by the quantity of fixed fire. Many more examples could be brought, but they might be thought unnecessary.

We could bring ten thousand examples to prove that the chemical action of bodies can set loose their phlogiston : The most common in nature is by fermentation. But there are others which nature is supposed to make use of, viz. the chemical fermentation of sulphur, iron and water. Many are of opinion that this fermentation, or chemical action, is often produced in the bowels of the earth, and is the cause of volcanos, earthquakes, &c. The violent action of these bodies with their phlogiston, which is evidently turned into actual fire, seems adequate to the effect. When lead and mercury are shaken together, their chemical attraction, aided by their attrition, decomposes their phlogiston into heat. In the operation, Dr. Priestley found, that he could scarcely touch them, the heat was so very intense.—See Priestley.

I need not have given myself so much trouble to contradict and confute these different extraordinary theories. It is, however, some satisfaction to hope that I have thrown great light upon the principles of combustion, founded upon Dr. Harrington's theory. For the overthrow of their other doctrines, I think I have an *experimentum crucis*. Mr. Lavoisier finds his theory upon the oxygen gas being imbibed into sulphur and phosphorus, in combustion. But (as our fathers in chemistry have judiciously

judiciously taught us) these bodies consist of acids and phlogiston.

If the marine dephlogisticated acid is mixed with essential oils, or with volatile alkalies, and fired in artificial dephlogisticated air †. The intenseness of the heat will decompose the dephlogisticated air, and cause it to be principally imbibed by the acids and phlogistic bodies, the same as in the combustion of sulphur. And, if the residuum is examined, the phlogiston will be found to have disappeared, forming actual fire, and the acids are left entire. What farther confirms the doctrine of phlogiston, is this; the marine acid having lost its dephlogisticated state, is left in the state of common marine acid, though it had imbibed, in the process of combustion, a quantity of oxygen gas.

Chemistry has of late suffered very great innovations; and, if our modern philosophical chemists go on as they have done, it is not known where they will stop. To me it has often been matter of surprise, that none of the old regular chemists have borne their testimony against such degradations. They all seem to be in a lethargic state, as if bitten by the *tarantula*.

One of the first principles of chemistry is that of chemical attractions; and our ancestors in

† What I mean by artificial dephlogisticated air, is that air which is made in the laboratory of chemists. For Dr. Harrington shows that this air is very different from the empyreal part of atmospherical air.

in this science persuaded, that these attractions are one of the most singular phenomena in nature, have handed down to us a regular table of them. Particular bodies have a very strong attraction for each other, very different from the principles of gravitation. If two bodies, having an attraction for one another, are exposed to each other's influence in the medium of a fluid, they will coalesce. Thus, if an acid and alkali are dropt into water, they will form a neutral salt: So also, whenever bodies are exposed to each other in a fluid or moist form, their attraction will take place. Fluidity seems to be necessary only to bring their particles into immediate contact.

But how are our present chemical attractions conducted by combustion? Combustion instead of allowing bodies to unite, acts as a strong mechanical power in breaking them down. On the contrary, according to their chemical attractions, combustion (which is the setting loose an essential part of most chemical bodies, viz. phlogiston or concentrated fire, let loose as actual fire) unites, but does not separate them.

Let us here give an example. I will take a salt, as it is a body, (the chemical principles of which we have been fully instructed in by our forefathers) and also an acid, with which we are well acquainted: Though this knowledge has been much obscured by some late chemists. If the volatile alkali is aerilized into an air or vapour, and after that exposed to the vapour of

the nitrous or marine acid, they will unite, so as to form the nitrous or marine ammoniac salt; and the heat which kept them in an aerial state becomes sensible. But if this alkaline air is exposed to still greater heat, it will then become inflammable air; and if the same nitrous vapour is added to this air, as in the former experiment, and then fired, it will emit an immense quantity of heat and light; even fifty times more considerable, than came from it in the former experiment; and upon examining the residuum, an acid water is found. Here the volatile salt has disappeared, and fire appeared, or was discharged in the operation. Indeed, in the former experiment no light comes from it, but only a small quantity of heat. Dr. Priestley says, vol. iii. p. 415. "Very little heat is produced by the union of acid or alkaline air and water, though, as I have found by experiment, there is *some* produced, whereas the decompositon of dephlogisticated and inflammable air, never fails to produce a very great degree of heat."

Then those who may be called our fathers in chemistry were convinced that the acid and volatile salt, by being united by a chemical attraction, would form a neutral salt, is now perfectly absurd; the true attraction does not take place till the combustion. And the true union of the alkali and acid is not a neutral salt, but water and an acid. Such absurd doctrines need no comment.

The

The very same farce has been acted in the experiment of firing the oxygen and inflammable gas. Water and an acid are in the residuum. The inflammable air, and also the fire, which neutralized the acid of the oxygen gas, have disappeared, as has been long ago fully proved by Dr. Harrington. The disappearing of the inflammable and dephlogisticated air, an acid and water being left in the residuum, is (as Drs. Priestley and Harrington found) the very same residuum as that which is left, when the nitrous vapour and inflammable alkaline air are fired. In one case our aerial philosophers cannot deny but that the acid which is left, came from the nitrous vapour and the water; heat and light, from the inflammable alkaline air. Therefore, by the same argument, we must equally allow, that the acid in the other experiment, came from the dephlogisticated air, and part of the water from the inflammable air. Nothing can be more plain and obvious. Yet our modern chemists will not allow these first and most common principles to be just ones; namely, that alkalis attract acids and neutralize them, and that phlogiston neutralizes acids \*.

If the neutral salt, which is formed by the marine dephlogisticated acid, and the fixed

H<sub>2</sub> alkali.

\* Dr. Crawford in his explanation of the nitrous acid deflagrating with combustible bodies, says, that the fire first set loose in the firing them, is the cause of this. And agreeably to him, this fire runs the gauntlet, of alternately aerilizing the oxygen gas of the acid, and of being consumed,

alkali are set on fire; as neither of these bodies are aerilized in the process, so there is less mystery in their operation. Before combustion they formed a neutral salt; but after the combustion the alkali has disappeared. What has become of it? We are altogether at a loss to answer this question according to their theory, unless they could introduce the art of *legerdemain* into their experiments of glasses, gun-barrels, &c. But here the aid of conjuration is still farther required; for, as one *demon* disappears, another in the shape of an immense quantity of light and heat makes his appearance. However, as the tricks of the juggler cease to surprise us when his art is known, so, had we not been so easy of belief and so fond of the marvellous, we would have paid a little more respect to what our chemical fathers have taught us; nay, I had almost said, what we may be convinced of by our own senses. It is hardly possible not to give our assent to this truth, namely, that combustible bodies, or phlogiston when fired, are changed into actual heat. By a proper attention to chemical principles, we should have believed, that phlogiston is concentrated or fixed fire. Yet it often happens, that as new discoveries please, merely because they are new, and every one is anxious to ascribe all phenomena

ed. To such ridiculous explanations are they obliged to have recourse to in support of a wrong theory. I think that such powerful phenomena is to be accounted for by such slight causes.

mena to them, so, upon the discovery of Dr. Black's elegant doctrine of latent heat, (which becomes latent in fluids, vapour, &c.) our modern chemists greedily grasped at it, and forced it head and shoulders into their present aerial fabric. But Dr. Harrington hath satisfactorily shown, that the heat in these phenomena is chemically united, the same as in alkalies and acids.

The idea and proof of heat or fire being chemically attracted by water, just as alkalies attract acids, (not a vague idea of its becoming latent) was the discovery of Dr. Harrington. And I take this opportunity to declare, that this, with many more of that gentleman's discoveries, have been mentioned by his enemies, without ever having glanced at, or even suggested his name. A behaviour in them so uncandid and ungenerous, impels me to make use of this pointed language, and to do all in my power to redress injured merit. I meant to have drawn a whole length picture of the unkind usage he has received, and of the unjust manner in which his discoveries have been treated, and with the plagiarisms of others from him. This however I am prevented from doing, as he informs me that he has been making observations for sometime, which he intends to publish. His enemies have acted a very unfair part; every one of them thought they had a right to buffet and pilfer from him at the same time. Though I could all along clearly observe, that his writings were regularly

larly operating upon the different chemical opinions; and that they are upon the eve of bringing about a general revolution in the chemical system. Great is the power of Truth, and she will prevail!

The present doctrines adopted by many chemists appear, when fairly investigated, to be so very absurd, that it is not possible to vindicate them. Heat (as Dr. Harrington observes) must be matter; unless we adopt Plato's opinion, who supposed it to be spirit. Then, (as in their experiments) we have clearly proved, that an immense quantity of heat and light comes from the combustion of the muriatic neutral salt, which could not possibly have been lying latent, as they call it, in the neutral salts, but must have been an integral part of the alkali: Besides this would be hostile to Dr. Black's doctrine, which is the basis of all the late doctrines of chemistry. For, in their experiments, a great generation of air and vapours instead of a condensation takes place; so that, as I observed before, were the present chemical doctrines brought in proof of a generation of cold instead of heat and light, they would have had some appearance of truth. And, let me ask this serious question: If an amazing quantity of matter comes from a body in the form of heat and light, is it not consistent with reason to suppose, that the light and the heat made a part of the body; and that, upon its being discharged, the body would be altered in its chemical principles?

To

To prove these principles, we can bring experiments, and set them in so clear a point of view, that we should think it impossible for any to mistake them. We have shown that the distinction between the dephlogisticated marine acid, and the common acid, is owing to the one being phlogisticated, and the other dephlogisticated. But to avoid disputation, let us suppose the difference to be conformable to Mr. Lavoisier's doctrine, the one possessing more of the oxygen gas than the other. And, with respect to the formation of alkaline air into inflammable, Dr. Austin has written a long paper in the Philosophical Transactions, in which he endeavours to show that it is performed by separating its inflammable from its phlogisticated air: It being the supposition of chemists, that the alkaline air is formed of these two bodies.

Then, to make experiments with all possible candour: Take the common marine acid air, and mix it with alkaline air, and they will form the common sal ammoniac: Whereas, take the dephlogisticated marine acid air, and fire it with alkaline air, and they will form, after the combustion, a watery marine acid. In the one operation, a great quantity of heat and light came from it; but in the other, comparatively little or no heat, and no light. This being so, our modern interpreters must say, that there was no chemical attraction in the first experiment, viz. the union of the common marine acid and alkali; but that it took place

place only in the second. Moreover, we rashly venture to correct the theories of our fathers, when we say, that alkalies and acids do not form neutral salts, but a watery acid. Here however we are still aground; for, in the process which left the residuum of an acid and water, there is an immense quantity of fire and light to be accounted for. You see, gentlemen, into what absurdities your own doctrines have involved you.

Dr. Austin in his laboured and very singular paper, built upon these hypotheses, supposes that when light, inflammable and phlogisticated air are fired in their compound state, as in alkaline air, they would form fixed air. In this case then, conformable to his hypothesis, alkaline air, and dephlogisticated marine acid, when fired, should form fixed air; but they do not.

That concentrated fire is phlogiston, may be proved, both analytically and synthetically. Dr. Harrington, in his Thoughts on Air, has proved the synthetic part. Of the many arguments he has brought, I would beg leave just to mention some of them \*. As to the analytical,

\* Dr. Priestley impregnated water with the vitriolic acid vapour, and exposed it in a glass tube to a continued heat in a sand furnace from the 9th of Sept. to the 20th of Jan. and the result was, as he says, more curious than he could possibly have expected, *a priori*. For the vitriolic acid had actually formed itself into sulphur, and some part approaching to it in the form of white crystallizations. "The crystals, (he says) I found were not dissolved in spirit of salt, and when

tical, we have, I hope, sufficiently proved it. Dr. Priestley found that heat would make the calces of metals become saline bodies; Dr. Harrington found that heat would make alkaline air inflammable; Mr. Scheele found that

when they had been washed and dried they had the colour and smell of sulphur; and being laid on a hot iron burned with a blue flame so as to leave no doubt of the identity of the substances." I make no doubt but if the operation had been long enough continued that all the acid would have been converted into these crystals of sulphur; for that which was left was particularly pungent, giving indication to have likewise received a greater quantity of phlogiston. I have by a longer process and greater heat entirely neutralized the acid, it being all in a manner changed into crystals. Dr. Priestley exposed the vitriolic vapour or air to this heat, and found the same crystals; and upon opening the tube one third of the air was gone, which no doubt had entered into the crystals (forming them with the heat.) But part of the air which remained would not unite with the water; therefore it implied that it had got a stronger impregnation of phlogiston. We cannot entertain the least doubt but that the acid had obtained phlogiston in these experiments, and it is equally clear that there was nothing to give it phlogiston but the fire. Mr. Scheele, p. 116. says, "Do we not observe that the vitriolic acid united with a small portion of phlogiston forms spirit of sulphur, and with still a greater quantity, sulphur?" P. 115. he likewise says, "Heat is united with certain substances, and makes one of their integrant parts." The nitrous and marine acids treated in the same manner form a white incrustation, which I have no doubt arises from the acid uniting with the heat, and forming a neutral union. The marine acid is supposed to corrode the glass, which may add to the incrustation; but the experiment which I think the most conclusive is this. I took the phosphoric acid, and exposed it for a long time to a strong heat, not by itself in the manner Dr. Priestley did, but diluted with water, and which will then even bear a red heat, as Mr. Scheele observes. After a long exposure it formed a number of crystals, all the acid

heat would make fixed oils volatile †. And Homberg found that the heat of the sun would make fixed alkaline salts volatile.

Gentlemen, for the sake of science listen to the voice of Truth, for though you have hitherto wielded the chemical sceptre with undisturbed sway, to point out your mistakes should not offend you, when done with candour, and to serve the cause of science. By being unwilling that your errors in chemistry should be rectified, is to endanger the loss of that character you merit from your labours.

To

acid being imbibed by them, and which upon examination appeared to be phosphorus, having all its qualities in some degree burning with that bright flame, and leaving an orange coloured stain; therefore we can have no doubt of the manner in which the phosphorus had regained the phlogiston, which it had before lost, by consuming in the open air. The crystals were so perfectly neutralized that they did not give the least acidity to water so as to have any effect in turning the juice of turn-sole red. Dr. Priestley found, that by exposing common vinegar to a long heat it became black and vapid, losing its acidity; both of which changes undoubtedly imply its neutralized heat forming it into phlogiston.

+ Substances which are united with a redundant heat, as alkalis, quick-lime, and litharge, attract the inflammable which is found in oily mixtures: They dissolve the fat oils and sulphur, and yield with it soaps. If you pour an acid to a saponaceous solution, the acid unites with the alkali, and sets the heat free; and since it becomes not sensible, it must undergo some new union; and meeting for that purpose with the oil, it unites with it; and the oil acquires by it the peculiar quality of becoming soluble in considerable quantity in spirit of wine, and to form with volatile spirits sal ammoniac prepared with quick-lime a peculiar kind of soap: Qualities which the fat oils likewise acquire after being distilled several times, when they likewise must have attracted heat from the fire. See Scheele, p. 174.

To say that your assent to the Harringtonian doctrine would confirm me the more in it, would be to say an untruth; for it would show great ignorance in me, when the two doctrines came to be fairly examined and compared together, to entertain the least doubt or hesitation, which is the true one.

Dr. Harrington shows, in the first publication of his theory, great penetration and a just knowledge of the world; for knowing the high characters he had to contend with, he says, "How difficult is it to establish truths against the prejudices of the day?" But he does not stand alone, as he has for his example the immortal Harvey, with respect to his discovery of the circulation of the blood. Though he gave so clear and plain a demonstration of his doctrine, that even a child's reason must have assented to it; yet his medical brethren cavelled at him with all their spleen and vehemence; And when they were forced to allow that he was right, they endeavoured to lessen his merit, by saying he was not the first discoverer.

But gentlemen, I have another experiment for you, which is, in my opinion, almost equal to Harvey's demonstration of the circulation of the blood. Mr. Kirwan says in his Essay, p. 31. "If sulphur be digested in oil of turpentine, and then slowly distilled for 10 or 12 days, it will be converted into vitriolic acid, according to Homberg. Mem. Par. 1703. Here it appears that the sulphur is first dephlogisticated, and then unites to the fixed air of the oil: It

evidently can receive no pure air from it. It must be remembered, that if this experiment be not cautiously conducted, it is very dangerous."

Here it appears that two high phlogistic bodies, by the action of heat, without either dephlogisticated air or acids, will have their phlogiston turned into actual heat. The oils, and likewise the phlogiston of the sulphur, are consumed, so as to form actual heat, there being a slow and gradual combustion. But, if the operator is not very cautious, he will be made sensible of the combustion, by the exploding of the vessels.

It is surprising to see the most clear and obvious facts wrested by their hypotheses. Mr. Kirwan says it is by the fixed air uniting with the sulphur, the sulphur being first dephlogisticated. Now, if we allow that the oil contains fixed air, what was to decompose it, what to dephlogisticate the sulphur, and what became of all the phlogiston? It surely must have acted the part of an invisible spirit. But by attending accurately to the experiment, you may sensibly perceive a slow combustion, with a separation of heat and light.

I cannot help observing, that our present chemists can, with the greatest facility, get over the most obviously plain facts, so as to press them into the service of their hypotheses; and certain I am, that when the truth comes to be known, chemists will then smile at their present wrong doctrines, and modes of reasoning.

Chemists

Chemists seem to possess a credulity that will receive any thing as true.

But as a farther confirmation of our doctrine, we will give an experiment more conformable to Mr. Lavoisier's. If dephlogisticated marine air is mixed with pure or respirable air, there will be a decomposition; and the marine acid, instead of now becoming more dephlogisticated, will become phlogisticated, or the common marine acid.

The hypotheses at present adopted by many chemists, have no regular table of attractions. They say, that if in some of their processes there is the least water, nitrous phlogisticated or fixed air, these bodies, under an intense degree of heat or cold, must be decompounded or compounded, and that at their pleasure; consequently their experiments are nothing, in point of chemical attraction, but a string of contradictions. Their leading elective attraction, is the oxygen and inflammable gas, or phlogiston. These two bodies, they say, have, in respiration, combustion, putrifaction, &c. the strongest attraction for each other.

All bodies which attract each other, when brought within the sphere of one another's attraction, unite when in a fluid state. But will not my reader be surprised when I inform him that the oxygen and inflammable gas, or *carbone*, so far from attracting each other, will not unite, when brought into a fluid state: The repeated trials made by chemists have not been able to accomplish it. Nay, the strong mechanical

chanical compression of the two gass by heat, and being long mixed together, is ineffectual; and yet they are fluids: Nor will it do, as in metals and oxygen gas, though the phlogiston and the oxygen fluid are condensed.

How comes it then that these should not unite, since chemists say, that it is by this attraction nature conducts all her operations? Nay, what is still more extraordinary; atmospherical air, though composed of dephlogisticated, and another kind of air, which (according to chemists) is supersaturated with phlogiston, will not (as Dr. Harrington observes) attract each other, nor unite; and yet they are not only blown from pole to pole, but even generated together in vegetables. Mr. Kirwan endeavours to reconcile this extraordinary absurdity, (for I can call it nothing else) by saying that the elementary heat of these airs prevents their union. The facts are in direct contradiction to this; for, dephlogisticated and nitrous air unite, and so do fixed and marine acid air unite with alkaline air. And besides these, many other examples might be adduced. He says farther, that for the same reason, fixed air will not unite with lime without the aid of water; and also, that concentrated oil of vitriol may be found on water, without uniting with it.

As to the averseness of fixed air to unite with lime, Dr. Harrington hath accounted for it, and brought a strong fact to prove that the fire of the lime is chemically united, and has a stronger

a stronger attraction for the lime, than the lime has for fixed air; but by the joint action of water, and fixed air on the earth of the lime, the fire's attraction is overcome. And as to the oil of vitriol, there is an oiliness, or repulsive quality, which checks the union, but that is overcome by the least motion: Therefore neither of these examples are in point.

It is said that Dr. Priestley made dephlogisticated and inflammable air unite, by means of a bladder forming fixed air. It is really matter of surprise and vexation to think, that such experiments should be once named, after what Dr. Harrington has said upon this subject: For, in the first place, these two kinds of air, upon their union, form either water or nitrous acid; and yet, in this case, they are said to form fixed air. Again, Dr. Priestley himself has acknowledged, that the bladder became putrid in the operation: And do not we all know, that animal putrefaction will turn dephlogisticated into fixed air. The inflammable air (conformable to Dr. Harrington) will aid the putrescence; and it is a fact not to be disputed, that bodies become putrid by attracting phlogiston; and by receiving the phlogiston of pure air they decompose it, so as to form it into fixed air\*. Then, gentlemen, the only union between dephlogisticated and inflammable

\* Why should a bladder form this union, if it were not by decomposing the air by putrefaction? For Dr. Priestley has endeavoured to unite them by every mechanical influence  
he

inflammable air, is effected by combustion, which indisputably acts, so as to set loose the concentrated or fixed fire of these two kinds of air: For, in all cases of bodies burning, their phlogiston turns into heat and light: And were there, as chemists would make us believe, so strong an elective attraction, we would certainly be able to accomplish it without the aid of burning: But this is positively nothing more than setting loose the phlogiston as actual fire. Of this we have a clear and positive proof in the neutral salt, which is formed of the de-phlogisticated marine acid and an alkali. Let me ask chemists, are they not chemically united, and is there not a chemical attraction? According to their doctrine, there is not.

Then let us bring in their favourite agent to produce attraction, viz. combustion. After combustion, the neutral salt has disappeared, and the residuum is an acid and water with an aerial expansion. Our fathers in chemistry, how egregiously were they mistaken! They were totally ignorant of chemical attraction. They supposed that an acid and an alkali would unite, and form a neutral salt. They knew nothing of combustion, our great chemical agent. They were such *ignoramus'es* as to suppose that a house is built by putting brick and mortar together. Our present aerial philosophers

he could think of; either by force, or by every singular chemical idea, which is, to admit their union by small quantities at a time; and this he calls their nascent state. Very extraordinary chemical ideas indeed!

losophers are much more knowing, they employ their agent, combustion, after the house is built. Hence, gentlemen, my fears are, that your agent, combustion, will become so general, as at last to blow up your doctrines. It is a dangerous thing to be too familiar with fire; therefore beware of the explosion.

According to Mr. Lavoisier's hypothesis, there is a very singular difference in the production of fixed from dephlogisticated air, and when water is produced from this air: The one being produced from *carbone*, and the other from inflammable air. But Dr. Harrington hath satisfactorily proved, that the difference in their production, is owing to the intenseness of the flame. That the nitrous acid and water are produced when it is strong; and when weak, fixed air.

With respect to the combustion of inflammable air, sulphur, phosphorus, &c. Mr. Lavoisier hath given some accurate experiments, which prove the heat to be very considerable; but the heat, when fixed air is produced, is considerably weakened in these combustions. The difference is, indeed, very great; but it seems evident that that must be owing to the intenseness of the combustion, and not to the particular ingredients burnt; as the same bodies burning produce the different residuum. In burning inflammable and oxygen gas, the fire is so intense, as to burn instantaneously, going off with a loud explosion; but when these two bodies burn in a

slow and gentle manner, they form only fixed air: To prove this, Dr. Priestley has given many experiments, and indeed, the proofs from them are very good ones.

But then, that his experiments may correspond with his very singular doctrines, he supposes that the fixed air is produced from the dephlogisticated air, by imbibing inflammable air in its nascent state, as he expresses it. To make the experiment, expose zinc, iron, red precipitate, turbith mineral, to fire in earthen retorts: And the process is simply this; the combustion takes place in a gentle manner, as the dephlogisticated and inflammable airs are generating; for, as the heat produces these airs, it will in consequence ignite them. That it is from this simple cause, is beyond all doubt; for, if you take care not to raise the heat so high as to ignite the airs, you will receive them both entire. Dr. Priestley was baffled in many of these experiments, owing to their firing with an explosion, after a quantity had been generated: However, he often produced them quite separate; which he could not have done, had there been (as modern chemists would have us believe) great attraction between the two airs \*. Now, these experiments

\* Again, I threw the focus of the lens upon red precipitate, in alkaline air, till three measures of it were reduced to two. Water was produced in the process, and the air that remained was considerably dephlogisticated; the standard of it, with a mixture of two equal measures of nitrous air, being 1.7.

But

ments are in direct opposition to Mr. Lavoisier's doctrine: For here is fixed air generated, when water only ought to have been generated, and there was positively no *carbone* ‡.

Nothing can more clearly establish our hypothesis, namely, that the quality of the residuum, after burning these airs, depends upon the intenseness and degree of the combustion;

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But in another experiment of this kind, there must have been a quantity of inflammable air set loose from the alkaline air, as well as dephlogisticated air from the red precipitate; because, after the operation had continued some time, there was a violent explosion within the vessel; which threw it many feet perpendicularly into the open air, as I was holding it in my hands. In this experiment I had been particularly careful to make every thing concerned in it as dry as I possibly could, in order to satisfy myself with respect to the production of the *water* which I had found before; and there was time enough before the explosion to observe that water was certainly produced in the process. To appearance the quantity of air was never much diminished. See Priestley, vol. vi. p. 194.

‡ Dr. Priestley, in order to prove that phlogiston and empyreal air, form fixed air, put bodies into a retort, which contained both empyreal and inflammable air, viz. red precipitate and iron, and he got a quantity of fixed air from them; from thence he concluded, that these two airs form fixed air. But the explanation of the phenomenon is this: The red precipitate, at first, yields its pure air, and the heat that the iron possesses at the time, with the action of the pure air upon it, sets it on fire, and there is an actual combustion. We need not be surprised at this, since the iron will burn under the same circumstances, out of the retort, viz. being exposed to pure air and a strong heat. The combustion in the retort is done in an imperfect manner; the pure air being generated so slowly, as not to produce that active ignition, as the fixed air to be decompounded, and be fixed by the calx of the iron. This is the case in other imperfect combustions.

for if a column of these two airs goes off instantaneously, here all the fire which these two airs possessed, acts at one and the same time, and must produce a considerable quantity of heat. But in those processes, where fixed air is formed, it burns in a gentle and gradual manner. What shows this in a most striking light is, if the inflammable air from the wood is fired with respirable air, it burns more gradually, so as to produce fixed air. Yet, if this same inflammable air has a greater quantity of fire thrown into it by the electric spark, or common fire, it will explode all at once, just as the inflammable air from metals does, and produce the nitrous acid.

And

combustions. Mr. Kirwan says, p. 85. "Mr. Morveau having left a piece of phosphorus in a large glass well stopped for 3 or 4 days, exposed to a temperature of 70 or 72°, and afterwards opened it in lime-water, the lime-water entered and became turbid, and being filtered, left a precipitate which effervesced with the nitrous acid, and consequently the precipitation did not arise from the union of the lime with the phosphoric acid. 1 Encyclop. p. 220."

Here, In both these cases, fixed air appears; but when there is a free ignition, the fixed air is decompounded, and enters into the calx in one case, and the phosphoric acid in the other, (the processes are directly the same.) But in experiments where the two metals have had a more active process, there has been, I have found, little fixed air generated, and it was found in the calx. And to prove that it is not by the union of these two airs (viz. inflammable and empyreal airs) in their nascent state, that they form fixed air, but from the combustion, I took zinc and the red precipitate, and with a gentle fire, so as not to produce ignition, I have formed these airs quite separate and pure. Or by adding water, either to the red precipitate and iron, or the precipitate and zinc, it will hinder the ignition, and in consequence, their bodies will produce empyreal and inflammable air.

And now I would seriously ask my reader, what is his opinion of that doctrine, which supposes, that such a body as phlogiston, can with dephlogisticated air, form fixed air, which is an acid? I say, with deplogisticated air, which shows no signs of acidity.

But if he (my reader) is so bewildered in the labyrinth of aerial philosophy, as not to know whether phlogiston is an acid or an alkali, I would beg leave to inform him, (and if he tries he will find it to be true) that alkalies, by means of more heat, may be formed into inflammable air; that the common alkali will equally explode with the dephlogisticated marine acid; and that oils, bitumens, &c. neutralize acids. If he wishes to have the authority of some eminent chemists, I will give him one of the first rank; Mr. Scheele says, It is an indisputable fact, that phlogiston neutralizes acids: And be it also known, that fixed air with water, exposed to the rays of the sun, will attract them, and lose its acidity, becoming pure air again. Nay, will not his surprise still increase when I inform him, that when this pure air is supposed to imbibe more alkaline air, the combustion being stronger, it will turn to the nitrous acid.

Let us consider these wonderful productions. Fixed air with water, by being exposed to the sun, becomes pure air; and by attracting a quantity of alkali or phlogiston, it becomes fixed air; or, as Mr. Bergman calls it, an aerial acid: And, by a greater quantity of the alkali, it

it becomes the nitrous acid. If the dephlogisticated nitrous acid is made use of in these combustions, the singular changes produced by its being fired in alkaline air, must appear still more extraordinary. We shall afterwards shew, that this doctrine is equally as absurd when considered in another point of view; for we will prove in the most evident manner, that this pure air, instead of receiving any addition in the combustion, really loses; as it becomes less heavy, and more contracted in its bulk.

That the rays of the sun or heat will phlogistigate acid vapour, is a truth so well known, and so properly established, that I need not enlarge upon it here. Dephlogisticated air is supposed to be the acidifying principle, and sulphur and phosphorus the substances which imbibe it. In combustion they become strong acids; and they are supposed to imbibe only as much of dephlogisticated air, as is equal to their own weight. Metals imbibe in their reduction, a smaller quantity of dephlogisticated air; and therefore, he supposes from that, that oxyds are not highly acesent: But there appears to be an evident deficiency in this doctrine; for, when dephlogisticated air is formed into fixed air, it must be all formed of air, or of the acidifying principle; for the fixed air is not equal to the weight of the empyreal air changed. This is the case in the combustion of all bodies, except charcoal, in the oxygen gas; and in that combustion, the fixed air which is formed is heavier than the empyreal air,

air, viz. 1-9 conformable to Mr. Lavoisier, and 1-5 conformable to Dr. Crawford: But conformable to them all, it is evident, that in the combustion of other bodies, the fixed air is lighter than the empyreal air.

This is a direct contradiction; for it is evident here, that it is much more probable to suppose, that the sulphur, phosphorus and carbone, are the acidifying principles, and not the oxygen gas; since the more of them is united with the oxygen gas, the more acid is the body; then the acid of fixed air is not to be compared in point of acidity with the vitriolic and phosphoric acids, and yet it should, conformable to their system †. They say, That oxygen gas is the principle of acidity; consequently, the acid of fixed air should be twice as strong as the vitriolic, since it contains twice the quantity of oxygen gas. But this is one of their many inconsistencies, which they can leap over with the greatest ease. Had Mr. Lavoisier paid a proper attention to the importance of these contradictions to his theory, he must have been convinced of its impropriety. In burning the variety of bodies, in nature, both of the animal and vegetable kingdoms, in oxygen gas, it is left in such a state, as to have both its diameter and weight decreased: And yet, most bodies in nature are supposed to abound

† And again, if the acidity of bodies depended upon the base which imbibed the oxygen gas, and not the oxygen gas itself, the oxyds of metals ought to be more acid, than even the acid of sulphur or phosphorus.

abound with fixed air; so that Mr. Cavendish endeavours to prove that the fixed air comes wholly from the body burned, and not from the oxygen gas.

The only body which, when burned, leaves a greater quantity of fixed, than the bulk of the empyreal air, is a factitious chemical body, made by art, called charcoal. And I would inform my reader, that this same charcoal, by being heated in a close vessel, and with the vapour of water, will give out a great quantity of fixed air. Mr. Lavoisier says, p. 86. "When the disengaged gasses are carefully examined, they are found to weigh 113.7 grs.; these are of two kinds, viz. 144 cubic inches of carbonic acid gas, weighing 100 grs. and 380 cubical inches of a very light gas, weighing only 13.7 grs. which takes fire when in contact with air, by the approach of a lighted body." Nay, there is no chemist but may know that charcoal contains a great quantity of fixed air: However, if his aerial experiments are so mysterious, as to have led him and others into errors, we shall give other examples so obviously plain, that it will be impossible to mistake them. The immortal Scheele says, p. 182. "The charcoal is most proper for the purpose, since it is a sulphur compounded of phlogiston and aerial acid. If coals be ground together with alkali, made caustic by quick-lime or fire, and then distilled in a glass retort in an open fire, a great quantity of inflammable air is thus obtained, containing no aerial acid: If a bladder

be

be tied to the mouth, the alkali on the other hand loses its causticity and effervesces with acids \*."

If other phlogistic bodies, even those which Mr. Lavoisier says are composed of *carbone*, will not, when burned with nitre, leave the alkali in a mild state; then these two essential points are sufficiently confirmed. First, That the fixed air resides in the charcoal before the operation; and secondly, That fixed air is not produced in combustion, by the oxygen gas of the nitrous acid uniting itself to the *carbone* of the combustible body: For, if that were the case, any combustible body, whose combustion would change pure atmospherical air into fixed air, ought, when deflagrated with the nitre, to leave the alkali in a mild state. Therefore, this simple experiment alone, viz. the deflating nitre with charcoal and other combustible bodies, should clearly convince Mr.

L Lavoisier

\* In the deflagration of nitre with charcoal, the coal is totally and rapidly decompounded. Its phlogiston and mephitic acid are suddenly let loose from their earthy basis, and from each other. The phlogiston combines with the nitrous acid, and instantly flies off with it in the form of *nitrous*, perhaps mixed with *atmospherical air*. Under this modification it had hitherto, particularly in the process for the *clystus* of nitre, escaped the inquiries of the chemists, till you detected it in the form of *air*. While the phlogiston of the charcoal is thus let loose, its whole mass of fixed air, or *mephitic acid*, being likewise suddenly disengaged, *but still continuing in its fixed state, or that of a concentrated acid*, rushes into the alkali deserted by the nitrous acid, and occupies its place. With it, it constitutes that *semi-neutral salt*, or that combination of alkali and mephitic acid, denominated Mild Alkali. See Bewly in Priestley's Appendix, vol. III. p. 391.

Lavoisier and others, that the nitrous acid is not decompounded in the combustion.

The process is simply this; the nitrous acid promotes the combustion by afflicting the fire to penetrate and set loose the phlogiston; for, as Mr. Cavendish observes, it gets united to a part of it, so as to form phlogisticated air. See p. 38 of this Treatise. Then, have we not a right to suppose that part of the fixed air in the combustion of the charcoal, comes from the charcoal itself, as it evidently possesses fixed air?

Were we even to allow Mr. Lavoisier's explanation of the manner in which fixed air is formed, when water is added to charcoal; yet it will not, I suppose, be contradicted, that in burning bodies in empyreal air, there is a small quantity of water which is deposited by the oxygen gas in the act of combustion. And Dr. Priestley has proved by experiments, that he found the attraction of charcoal for water, when heated, to be so great, as even to steal it from the leathers in his airpump, and with it to produce fixed air. Now, under this review of the subject, can the burning of charcoal be brought as a fair example? Nay, even in respiration, the fixed air is less in weight than the oxygen gas.

I adduce an example, and strange will it be, if it is not admitted as a proper one (though, to inform my reader, as matters are managed by chemists, the not admitting it would not surprise me.) The electric spark will change the pure part of atmospherical air into fixed air.

But

But is charcoal this electric spark; or will Mr. Lavoisier name this as a discovery? This spark, conformable to his doctrine, must be charcoal or *carbone*. According to him, charcoal is the purest *carbone*; for he says, p. 83. "After the operation is finished, we find nothing but a few atoms of ashes remaining in the tube EF; the 28 *grs.* of charcoal having entirely disappeared."

As the quantity of fixed air is considerably less, when all bodies are burned in empyreal air, to what it is when charcoal is burned in it; so to account for this phenomenon, they say there are two kinds of combustion, one the *carbone*, and the other inflammable air; which latter combustion is supposed to produce water. The present chemical philosophers are never at a loss to account for phenomena.

The combustion in the burning of inflammable air, sulphur and phosphorus, is very intense, considerably above the heat of burning charcoal, as we have shown; and therefore it is with great propriety that Dr. Harrington lays down this general rule, that the air is decompounded in proportion to the intenseness of the fire. If very intense, it is decompounded to its original ingredients, fire, an acid and water; if not so intense, it retains its aerial form in the state of fixed air. This (as he shows) is seen in a striking light in the burning of phosphorus; to which, if air is only gently added, the combustion will be very imperfectly produced, and the air only changed into fixed air.

Now, as the burning of charcoal is not very intense, so it produces only fixed air. The burning of wood is considerably less intense than that of charcoal; therefore, we should not expect that there would be, in this combustion, any hydrogen gas decompounded. Mr. Lavoisier found the burning of hydrogen gas to exceed the burning of charcoal, as 52.16280 to 37.52823, in one pound of oxygen gas. Hence, as the burning of wood produces so little heat, we cannot suppose there is any hydrogen gas produced in the process.

But then, much less fixed air is found in the burning of wood, than in the burning of charcoal; the oxygen gas being rather decreased in weight and measure by the process; we have no right therefore to suppose there is any burning of hydrogen gas in wood! Charcoal itself gives out a purer inflammable air, by distillation than wood does; yet, they are both the heavy kind of inflammable air, which, when burned, turns the oxygen gas into fixed air. And Dr. Harrington has clearly shown, that this inflammable air is chiefly produced from the fire in the act of distillation; wood being composed of an acid and earth necessary to concentrate fixed fire: Moreover, after what Dr. Harrington has said, I should hope, that the absurd doctrine of oxygen and hydrogen gas, as forming water, is given up,

As chemical bodies, such as charcoal, do in general consist of gross bodies, the chemical properties of which we are but little acquainted

ed with, we shall not insist on them. Let us take the pure electric fire, so general and abundant in nature, and consider its properties and operations on air. Electric fire acts upon bodies in the same manner that common fire does; it sets fire to combustible bodies, dissolves iron and calcines it: It increases the bulk of alkaline air, and makes it inflammable. All these effects are peculiar to fire; but they are much stronger than those of any fire which art can produce. Electric fire in passing through a bar of iron will melt it, though its passage is so wonderfully quick, as to be performed in an instant of time. Now, we are certain, that its power to melt the iron at once, must be from its fire; and no fire that we can produce will have the same effect upon the iron, till after a very tedious operation. Iron can likewise be calcined by it in the same quick and wonderful way.

Let us consider then, its action upon respirable air. Dr. Priestley found that electric fire can turn the dephlogisticated part of atmospherical air into fixed air. If then, as is really the case, dephlogisticated air is turned to fixed air, without the agency of the third body, viz. a combustible one; this is a direct proof of our theory: The intenseness of the electric fire being sufficient to set loose the air's fixed fire: And that the air's fixed fire is set loose, is discoverable by the spark being so much more vivid, when taken in atmospherical air, to what it is when taken in any other air, or in

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*vacuo.* Dr. Harrington hath fully elucidated its action upon air ‡.

Mr. Cavendish, in defence of his theory, says, that the fixed air produced by the electric spark, is from the combustion of some extraneous body in the operation. But, as a confutation of this vague supposition, it may be sufficient to observe, that the same apparatus will,

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‡ We come next to Mr. Cavendish's important experiment in forming the atmospherical and dephlogisticated airs into the nitrous acid; but we shall give a different explanation of it, from what Mr. Cavendish has done. I have all along supposed that respirable airs are formed of an acid, fire, and water; and that the atmospherical air is a very different air from the artificial empyreal air of chemists: the latter not at all supporting vegetable life, and likewise animal life being not able to injure it completely, as animals died in it when it was better by the nitrous test, than the purest atmospherical air.

Hence in Mr. Cavendish's experiment, when the atmospherical and artificial empyreal airs are mixed, there is a quantity of actual fire thrown into them. We have before proved, that fire will decompose respirable air, and according to the intensity of the fire, the air will be accordingly decomposed. If it is the common combustion, such as wood, charcoal, &c. the air will be left in the state of fixed air; but if it is a more intense heat, such as sulphur, phosphorus, metals, &c. the air will be decomposed into an acid and water.

Now, the electric heat is a most intense one, it being capable of producing a phenomenon that we cannot imitate by any chemical process. Metals are only melted by heat; the electrical fire is so wonderfully intense, as to dissolve iron in one discharge of the battery. Dr. Van Marum melting a considerable body of iron at one discharge; he likewise entirely calcined 24 inches of leaden wire, three-eights of an inch in diameter, by one explosion. This shows a degree of heat we cannot imitate. Our heat that we can apply, requires a long continued application, but this is instantaneous. Therefore,

if ever so long continued, equally produce the very same effect, and with golden wires to conduct the spark. Now, if there were any accidental combustible body at the first part of the operation, it would in time be consumed ; but the effect equally continues. Moreover, if the spark is taken in artificial dephlogisticated air, its fixed fire being more closely united, the spark will not be able to decom-  
ound

Therefore, when empyreal air is acted upon by this wonderful heat, we should expect it would decompose the air to an acid and water.

This theory is most strikingly shown, in firing inflammable and empyreal air. If there is a full proportion of inflammable air, there will be such a degree of fire set loose, as to fly off with the acid, as we have before explained ; but if there is a less proportion of inflammable air, so as not to produce so great a heat, the acid will be left in the residuum. A similar phenomenon will take place in the burning of the respirable airs in the electrical fluid. If the electrical fluid is considerable, and the empyreal air very rich, part of the acid will be carried off in the same manner as when burned in inflammable air. But if the electrical fire is not so considerable, and the empyreal air not so rich, far more of the acid will be left in the residuum. This will account for the different results between Mr. Cavendish's and Dr. Van Marum's experiments ; the latter using a greater quantity of the electrical fluid, and a richer air than the former.

Mr. Cavendish supposed that the acid came from the phlogisticated air ; this arose from their theories, which supposed that there was no nitrous acid in empyreal air. But he found that he could not obtain any nitrous acid in this process, when he used only phlogisticated air, and he could obtain some when he used empyreal air ; but this he imputed to the impurities of that air ; however, there is more of it decompounded, than can be accounted for by that cause : But when he mixed the atmospherical air with the artificial empyreal air, he then could form them into the nitrous acid with the electrical fluid.

I have

pound it, and consequently no fixed air is produced; whereas, were it produced by some combustible body being ignited in the operation, the dephlogisticated air, instead of restraining, would make it burn with more vividness: It being a well known fact, that bodies burn more intensely in this air than in atmospherical air.

But

I have all along (in my publications) shown the atmospherical air to be a very different air from the artificial respirable air made by chemists. The former is made by nature's own delicate hand; her ingredients being water, fixed air, and the sun: the latter being made in an laboratory, with mineral acids, water, and earth, or salts, forced into an aerial form by an intense fire. The one will support both animal and vegetable life, but the other will support neither of them; vegetables dying immediately in it, and animals long before they have much injured it, even when it is in a far purer state than the best atmospherical air.

Hence in this experiment of Mr. Cavendish, the electrical fluid is not able to burn so considerably in empyreal airs, so as to reduce them, except the atmospherical air makes a part of them. By the atmospherical air, the electrical fire is so ignited, as then to operate forcibly upon the artificial empyreal air, and to produce so strong a degree of heat, as to decompose the airs.

This artificial empyreal air, we have likewise all along shown to be of a strong combustible quality, burning with partial explosions (according to Dr. Priestley, as if it was full of some combustible matter). But by adding a greater quantity of phlogiston or concentrated fire to it, I have made it so inflammable, as to explode all at once.

These two airs then, viz. nature's atmospherical air, and the high concentrated artificial air of chemists, being mixed together, and exposed to the influence of the electrical fire, will be decomposed, producing the nitrous acid; but the acid is not produced from the decomposition of the phlogisticated air. For take only the empyreal part of the atmospherical

But all the phenomena produced by the electric spark, when taken in atmospherical and artificial dephlogisticated airs, are to be accounted for only by Dr. Harrington's theory. When these produce the nitrous acid, it is conformable to our theory, which also fully accounts for the phenomena being from the artificial dephlogisticated air acting as the combustible body, to atmospherical air as the agent;

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agent;

rical air (which is easily done, by expelling the air from water; and then exposing the water to the atmosphere, and then expelling the air again:) After that, mix it with the purest artificial empyreal air, and there will be the same phenomena.

Or take the phlogisticated part of the atmosphere only, and mix it with the purest empyreal air, and there will not be these phenomena. No doubt some acid will appear, but it will be no more than what would have been, if the phlogisticated part of the atmosphere had not been added to the artificial empyreal air; for the higher the concentration of fire in aerial bodies, the greater difficulty there is in decomposing them. Thus in animal respiration, I can decompose the whole of the empyreal part of the atmospherical air, and only about one third of the artificial empyreal air, and that with difficulty; so that instead of its being a more luxuriant air for animal life, it is the reverse; this is discovered by your breathing in it; and very sensibly in the respiration of mice, that delicate animal is seen to have the greatest difficulty of breathing in it from the first, and dies before it has half injured it.

But even inflammable air may be breathed and decomposed; the immortal Scheele reducing a great quantity of it to foul air, by breathing it alone. Therefore if the artificial empyreal air is exposed to the electrical fluid itself, it will in some measure be acted upon as we might expect. But Dr. Priestley has shown us long ago, that the empyreal part of the atmosphere may be all decomposed, by the electric spark.

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agent; indeed they elucidate one another. The electric spark is capable of being ignited only in atmospherical air; and by the great heat which comes from it, a considerable quantity of the fixed fire of the artificial dephlogisticated air is set loose, by which the fire becomes so intense as to decompose the airs; and by taking them from their aerial forms, to produce the nitrous acid. That phlogisticated air has

This then is the true explanation of the experiment; and the acid which is deposited in the experiment, comes from the empyreal air.

In making the red precipitate, we may be certain in what manner empyreal air is formed. The red precipitate, and the mercurius calcinatus, both yield empyreal air, though they are formed by very different processes. We have before supposed, that mercurius calcinatus received its empyreal air from the atmosphere; but we can by no means suppose the red precipitate to do the same.

I took a quantity of the calx of mercury, formed by the nitrous acid, and after drying it, I examined it, and found it to be the calx of mercury and the nitrous acid. I then exposed it to a gradual degree of heat in a retort, and collected whatever was distilled from it; the heat was added very gradually to it, and I observed it regularly changed its colour to a yellow, and after that, to a bright red, called the red precipitate. After having formed the red precipitate, I examined if any thing had been separated in the course of this process from the salt, but I found nothing. In this experiment which I made at different times, I found if I pushed the heat strongly at first, I would get a little nitrous acid from it. But if the salt is well dried before the experiment, and the heat gently and gradually applied to it, nothing will be separated.

The bodies then which compose the red precipitate, are the calx of mercury, the nitrous acid, and fire. That fire should have this power in uniting with it, and changing it to red, is what we might have expected; as Dr. Priestley has long

has nothing to do in the process, is clear from this, that the electric spark, without atmospherical air, is not able to produce the nitrous acid from artificial dephlogisticated and phlogisticated airs. Moreover, as Dr. Harrington observes, if you take the pure part of atmospherical air, excluding the impure or phlogisticated part, it will equally, with artificial dephlogisticated air, produce the nitrous acid.

Would but our aerial chemists attend to reason, every doubt about the truth of this doctrine might be removed by the following fact. The electric spark will produce fixed air, when taken in atmospherical air. Now, need I inform chemists, that in most combustions, dephlogisticated air is turned to fixed air; that when the combustion is more intense, it is

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long taught that the nitrous acid becomes red, and phlogisticated by fire or heat; and that the mercurial calx has so great an attraction for fire or heat, that it will be even reduced by it alone: Nay, the phosphoric and the vitriolic acids will form phosphorus and sulphur with it (see Priestley). But I have unquestionably proved that an earth and an acid have, when united, the strongest attraction for heat; therefore the mercurial salt in this experiment, will certainly saturate itself with heat, producing the red precipitate.

Here then is empyreal air formed in the mercurial calx, it being just the same calx as that which imbibes the atmospherical air, viz. the *mercurius calcinatus per se*. They will both, if exposed to great heat, discharge their empyreal air and be reduced, or not, according as the fire is pushed. But, as we have shown all along, that the atmospherical and artificial empyreal airs are very different airs, so they will make different calces; it is well known to chemists and physicians, that these two calces, viz. the red precipitate, and the *mercurius calcinatus*, are different. See Harrington's Letter, p. 126.

turned to the nitrous acid, as in the combustion of dephlogisticated and inflammable airs : Nay, Mr. Cavendish says he actually turned atmospherical air into the nitrous acid, and not fixed air, in this same experiment. Dr. Priestley, I think, need not to be told this, since he has followed Dr. Harrington in proving it; though from an illiberal policy, he has omitted to mention that gentleman's name : But time which exhausts all things, truth excepted, strengthens those doctrines which are founded upon just principles. However, the Dr. has this salvo ; viz. he must differ from Dr. Harrington in supposing (conformable to the general hypothesis of chemists) that in the process, the nitrous acid must be compounded of inflammable and dephlogisticated airs, as water is supposed to be produced by these airs ; but such a supposition is from not reflecting that inflammable air is principally fixed fire, which escapes in the combustion, together with the fire that neutralized the acid in dephlogisticated air. Dr. Harrington hath, in the most public manner, called upon Mr. Cavendish, either to acknowledge the truth of his theory, or to defend his own : And certainly it becomes that gentlemen to do it publicly.

Dr. Beddoes says, p. 5. " Nitrous acid has been procured by making vital air from manganese pass along with atmospherical air thro' an heated tube, though the experiment has not yet been sufficiently varied, nor repeated, to be published at large, and with names." To strengthen

strengthcn therefore our arguments concerning heat if necessary, we may observe that heat is able to accomplish what electric heat can do; for it appears that a heated tube will equally operate on these two airs, so as to produce the nitrous acid.

Thus we have the strongest proofs in favour of our theory of combustion; viz. that the combustion of bodies is always in proportion to the degree of intenseness of the fire: For, when the intenseness is considerable, as in the electric spark, it will even decompose or set free the fire of atmospherical air. So alio, even common fire in a heated gun barrel can decompose fixed fire, when the concentrated artificial dephlogisticated air is added to the air of the atmosphere: But when a combustible body is added to atmospherical air, it will easily set them on fire, or decompose their heat; because atmospherical air, when acted upon by the intenseness of heat or fire, which is produced from the ignition of a combustible body, is soon ignited: Both acting together, viz. the heat produced from the ignition of atmospherical air, and also that from a combustible body.

Combustion may be thus defined: The fixed fire of bodies can be set loose or made actual fire, only by being acted upon by a very high degree of actual fire; and then only upon a combustible body, and its agent atmospherical air. The agent seems to act by having its acid and fire more loosely attracted, and therefore the

the easiest set free; which then acts upon the combustible body. This is evident from the fire of the electric spark being able to set loose the fixed fire of atmospherical air: But it is not even able to set loose the fire of artificial dephlogisticated air, when it is more fixed or concentrated. Moreover, as to the fixed fire in iron; though iron greatly attracts the electric spark, yet unless it acts at the same time upon atmospherical air, nature's great agent, it will not consume or let loose the iron's phlogiston, but only melt it: But if they both act together, the iron will be calcined, and the air decompounded, because the fire of both becomes actual.

This theory of ours accounts in the most beautiful manner for the fire produced by attrition: For, as we have already observed, the fixed fire of bodies is chemically attracted; and we have likewise shown from Dr. Darwin's experiments upon the expansion and attraction of atmospherical air, that mechanical force can resist the chemical attraction of heat. It discharges heat by compression, and attracts it by expansion. Here then it evidently appears, that mechanical pressure overcomes chemical attraction: However, I do not mean to prove by these experiments, that it is the actual attrition which separates the heat; but only, that air attracts a certain quantity of fire conformable to its expansion; and by altering the expansion, you alter its quantity of fire: *i. e.* Mechanical compression overcomes chemical attraction.

attraction. But what affords a most ample explanation and illustration of our theory is, the sparks produced by the mechanical attrition of flint and steel, when struck, the one against the other. These two bodies contain a great quantity of fixed fire; and being of a hard substance produce much friction in coaction; but if they are struck together in *vacuo*, no fire or spark will be produced: And upon the same principle, and without the aid of the same agent, atmospherical air, the electric fire will not produce the ignition of iron.

Mr. Cavendish and Dr. Priestley seem to have had different results, from the passing of the electric spark through common air. The former thinks he decompounded the air into the nitrous acid, and the latter is of opinion, that the common air was changed into fixed air. These are the two first experimenters, and therefore deserve the greatest credit. I can easily suppose, that the atmospherical air would be acted upon according to the intenseness of the spark; if then a small spark, it might produce only that degree of heat or combustion, capable to generate fixed air; but if more intense, the nitrous acid would be the result: And the observations I have made upon the experiments favour this conclusion, which is perfectly conformable to our theory of combustion \*. The only observation that can be made

\* The foregoing experiments show, that the chief cause of the diminution which common air, or a mixture of common  
and

made from this experiment of Mr. Cavendish, just named in the note, is, that the electric spark will even decompose a part of artificial dephlogisticated air; for, had Mr. Cavendish operated upon a purer kind of artificial dephlogisticated air, he would have decomposed a part of it. The decomposition depending upon the concentration, or fixed power of the fire

and dephlogisticated air, suffers by the electrical spark, is the conversion of the air into nitrous acid; but yet it seemed not unlikely, that when any liquor, containing inflammable matter, was in contact with the air in the tube, some of this matter might be burnt by the spark, and thereby diminish the air, as I supposed in the above-mentioned paper to be the case. The best way which occurred to me of discovering whether this happened or not, was to pass the spark through dephlogisticated air, included between different liquors: For then, if the diminution proceeded solely from the conversion of air into nitrous acid, it is plain that, when the dephlogisticated air was perfectly pure, no diminution would take place; but when it contained any phlogisticated air, all this phlogisticated air, joined to as much of the phlogisticated air as must unite to it in order to reduce into acid, that is, two or three times its bulk, would disappear, and no more; so that the whole diminution could not exceed three or four times the bulk of the phlogisticated air: Whereas, if the diminution proceeded from the burning of the inflammable matter, the purer the dephlogisticated air was, the greater and quicker would be the diminution.

The result of the experiments was, that when dephlogisticated air, containing only 1-20 of its bulk of phlogisticated air (that being the purest air I then had), was confined between short columns of soap-lees, and the spark passed through it till no further diminution could be perceived, the air lost 43-200 of its bulk; which is not a greater diminution than might very likely proceed from the first-mentioned cause; as the dephlogisticated air might easily be mixed with a little common air while introducing into the tube. See Cavendish in the Philosophical Transactions, vol. lxxv. p. 382.

fire of the dephlogisticated air, and not at all upon the phlogisticated air employed.

He says, he even decompounded phlogisticated air; but I much doubt the capacity of this air for decomposition. He admitted liver of sulphur to it, which, when under a great fermentation in absorbing dephlogisticated artificial air, I have known it absorb phlogisticated air, the same as charcoal. But perfectly ascertained of this fact, we may conclude, that if the electrical spark produces fixed air, it must be by decompounding the dephlogisticated air: And, a proper attention to Dr. Harrington's experiments and arguments, and to Dr. Priestley's, who has followed him, must convince us that dephlogisticated air, when decompounded by an intense combustion, (viz. in the firing of inflammable and dephlogisticated airs) produces the nitrous acid, without the aid of phlogisticated air.

And, if the experiment is repeated with atmospheric air only, the electric spark will decompound it, sometimes into fixed air, and sometimes into the nitrous acid. This reconciles the different results of Mr. Cavendish and Dr. Priestley, and can no otherwise be accounted for but by our theory.

I have, I hope, sufficiently proved that Mr. Lavoisier has entirely deserted Dr. Black's doctrine; and yet, in the beginning of his nomenclature, he opens his introduction with supposing that all vapours are the same as airs, and that they contain their heat in the same

manner ; he explains his hypothesis by that evaporable fluid ether. Ether, he says, when the weight of the atmosphere is taken from it, will become vapour at the heat of  $32^{\circ}$ . Then, agreeable to this hypothesis, the caloric, which the ether possesses, is what goes to aerilize or evaporize it. When condensed in water, it shows very little heat ; but when fired, an immense quantity of heat and light comes from it : And this, he says, is principally owing to the heat which evaporized it. But, that the heat which evaporized, has little to do in the process, is proved from the well known experiment of a few drops of ether being suddenly dropt into atmospherical or dephlogisticated air ; for this is no sooner done, than (if they are set on fire) there will be an immediate and vehement explosion. If the ether receives any heat, it must be from the air ; because the operation being suddenly performed, it has no time to take any heat ; or at least, what it does take must be from the atmospherical air.

Mr. Lavoisier would lay down this general hypothesis, and make it pass for true doctrine ; that caloric repels the particles of bodies, like water expanding a spunge ; and that the heat which this water attracts, upon the common principle of vapour, is the cause of our atmosphere : Nay, he supposes, conformable to his hypothesis, that the atmosphere is composed of a heterogeneous mixture of all bodies, even of metallic ;—such an imagination has more of the *air of romance* in it, than of the principles of

of true philosophy. The caloric of ether then is from the heat imbibed in the evaporation; and the caloric which comes from it, when burned, is not owing, in the least, to the spirits of wine of which it is formed. But Mr. Lavoisier, with all his narrowness of caloric to ether, acts an opposite part to the nitrous and dephlogisticated marine acids: For, unmindful of this smooth theory of his, he forgets his state of aerial vapour, and will have all the caloric that is produced from the burning of nitre and sulphur, to come from the condensed gas in the acid. Gentlemen, Does such a hypothesis require a refutation? A little serious reflection may convince you that it refutes itself.

Mr. Lavoisier begins his chapter upon the analysis of atmospherical air, with saying, "From what has been premised, it follows, "that our atmosphere is composed of a mixture of every substance capable of retaining "the gaseous or aeriform state in the common "temperature, and under the usual pressure "which it experiences. These fluids constitute "a mass, in some measure homogeneous, extending from the surface of the earth to the "greatest height hitherto attained, of which "the density continually decreases in the inverse ratio of the superincumbent weight. "But, as I have before observed, it is possible "that this first stratum is surmounted by several others consisting of very different fluids."

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The only comment I would make upon this paragraph is, that the word *beterogeneous* should be used instead of *homogeneous*.

The experiment which first suggested, and is the foundation of Mr. Lavoisier's doctrine, appears, upon the first glance, to be very much in favour of it; and, as he says, to prove it both analytically and synthetically. If mercury is calcined in atmospherical air, the air will lose its oxygen gas, being imbibed by the mercury; and, upon reducing the mercury, a quantity of oxygen gas will come from it; the mercury will be reduced, and the air become respirable again. Now let us investigate these phenomena, and see whether Mr. Lavoisier's theory, or that of Dr. Harrington, will best account for them.

Mr. Lavoisier says that the oxygen gas is a pure element. Dr. Harrington says atmospherical air is formed of fire, fixed air, (or, as Mr. Bergman with greater propriety calls it, the aerial acid) and water; but the factitious oxygen gas of an acid, water and earth. Now Mr. Lavoisier allows that the reducing of metals is an act of combustion, which is strikingly seen in the reducing of iron by burning. And Mr. Lavoisier must also allow that, when most bodies are burned in atmospherical air, the oxygen gas turns to the aerial acid or fixed air; and that, into whatever this oxygen gas is turned in the burning of mercury, it is imbibed by the calx. I should expect that Mr. Lavoisier will allow me to suppose, that the combustion

combustion of one body is much the same as the combustion of another, (just as the respiration of one animal is like to that of another) that is, acts the same upon oxygen gas. Then I will suppose that the burning of the mercury, and most of the bodies upon this earth, is the same, and that they all turn oxygen gas to an acid air. In one case only, the oxygen gas is not imbibed by the burning body, but in the other it is. Then let us suppose that this fixed air, or aerial acid, is imbibed by the calx of mercury. That the calx of mercury has a great attraction for air, is a fact which has been long known in chemistry. I will give Mr. Lavoisier an example. The corrosive sublimate of mercury, if united to an alkaline salt, the *sal absinthii* for instance; the marine acid will leave the calx of mercury, and unite itself to the alkali; and the fixed air of the alkali will unite itself to the calx of mercury. This experiment is well known to chemists: And it is equally well known, that if this calx of mercury is exposed to heat, it will form oxygen gas, and the mercury will be reduced. Dr. Harrington says in his Letter, p. 88. " But even fixed air may be formed into " pure dephlogisticated air, if united with the " calx of mercury (this calx we have all along " proved to have the greatest attraction for " concentrated fire) viz. in the experiment of " the corrosive sublimate being decomposed " by an alkali; if exposed to fire, it will yield " empyreal air; but it cannot bear the expla-  
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" nation that Mr. Kirwan gives of it, viz. the  
" fixed air being decompounded; for if the fire  
" is not pushed, you will get the dephlogisti-  
" cated air from it, and the calx will not be  
" reduced: Nay, to shew that the calx is not  
" phlogisticated after this process, agreeable  
" to Dr. Priestley's own test, it will form with  
" the nitrous acid, pure dephlogisticated air  
" again; or if united with the marine acid,  
" and then precipitated again with the alkali,  
" it will form again pure dephlogisticated air.  
" Our theory here appears so rational, that  
" when this dephlogisticated air is burned, it  
" will be decompounded; that is, its fire will  
" be separated from the fixed air, both being  
" produced in the process of burning."

Now, it is evident that combustion, or the setting loose a quantity of actual fire, has the power of turning the oxygen gas to an acid; and we must suppose that the aerial acid, when condensed in the mercury, must become concentrated, forming an acid of much greater acidity. That fire will turn the pure part of atmospherical air into fixed air, has been fully shown in this letter; viz. the electrical spark taken in atmospherical air; and it will also turn oxygen gas into the nitrous acid. It has likewise been proved, that when nature's oxygen gas of the atmospherical air, and the factitious oxygen gas of chemists, are operated upon by pure fire, (see p. 84. of this Treatise) that they will be turned to the nitrous acid.

Then

Then we are clearly brought to this conclusion, that acids and the calx of mercury produce oxygen gas; and what directly confirms this conclusion is, that if an acid, either of nitrous, vitriolic, or fixed air, is added to the calx of mercury, they produce oxygen gas.

Chemists say, that this production of oxygen gas, is from the oxygen gas, which they suppose the acids and the calx possess. But with respect to those other bodies which are said to possess oxygen gas, try if they will produce oxygen gas with the calx of mercury; and steam, as they say, is capable of decomposition; then water (for instance) seems to be the most proper body, as containing the greatest quantity of oxygen gas: And likewise the calx of mercury has a strong attraction for its inflammable air. Water then, conformable to them, should be the most proper for the calces to get oxygen gas from; and therefore the properest body to unite to the calx of mercury: But it will not answer to unite the calx with this nor any other body, but only with acids. Moreover, what strikes directly against their hypothesis is; the factitious oxygen gas is fully proved, by Dr. Harrington, to be a different kind of gas, from the pure part of atmospherical air.

Then, in this case, we must be allowed to say, that the bodies, necessary to produce oxygen gas, are an acid, the calx of mercury and water. It is well known that acids and the earth of metals have a strong attraction for each other; and these two bodies have a strong attraction

traction for fire or phlogiston. The nitrous acid, by being exposed to a great heat, becomes red and phlogisticated ; and the calx of mercury has so strong an attraction for fire, that heat alone will reduce it. And, as to phlogiston, the nitrous acid has so very strong an attraction for it, that chemists have placed these two at the head of the table of attractions ; and the calx of mercury greatly attracts heat and becomes reduced. It is no less true that heat will aerilize both the nitrous acid and the mercury.

Now, under a review of all these circumstances, are we not authorized to suppose that, when these bodies, the acid, the calx and water, are for a long time exposed to a great heat, that they will fix a quantity of this heat or fire, and be aerilized with it, forming that neutral phlogisticated body, called oxygen gas. The action of the fire implies this ; for when they become red, they give out only empyreal air : And it is very well known that fire or phlogiston reddens the nitrous acid.

That the acid gets neutralized with the fire and water, appears from Mr. Cavendish's experiments \*. This process is still more evident

\* A strong confirmation of this is, that red precipitate, which is one of the substances yielding dephlogisticated air in the greatest quantity, and which is prepared by means of the nitrous acid, contains in reality no acid. This I found by grinding 400 grains of it with spirits of sal ammoniac, and keeping them together for some days in a bottle, taking care to shake them frequently. The red colour of precipitate was rendered pale, but not entirely destroyed ; being then washed with

dent in lead. If lead is burned in a quick way in atmospherical air, it will form only the grey calx of lead, as it imbibes the acid of the air in its reduction: And if the calx is exposed to a great heat, it will give back again only that acid. But if this lead is calcined in a gentle way, by being exposed to the reverberation of the heat, it becomes gradually red, till at last the red is highly florid, and of the same colour as the calx of mercury.

That the calces become red from the fixed fire in them, is strongly confirmed by this, that their colour is similar to that which the blood receives in the lungs; this has been fully proved by Dr. Harrington, to proceed from no other cause, but receiving fire or phlogiston: And what farther corroborates this opinion is, that alkaline salts, which I hope, I may now be allowed to call fixed fire, will produce the very same effect upon the blood, forming it

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with water and filtered, the clear liquor yielded on evaporation not the least ammoniacal salt.

It is natural to think, that if any nitrous acid had been contained in the red precipitate, it would have united to the volatile alkali and have formed ammoniacal nitre, and would have been perceived on evaporation; but in order to determine more certainly whether this would be the case, I dried some of the same solution of quicksilver from which the red precipitate was prepared with a less heat, so that it acquired only an orange colour, and treated the same quantity of it with volatile alkali in the same manner as before. It immediately caused an effervescence, changed the colour to grey, and yielded 52 grains of ammoniacal nitre. See Cavendish in the Philosophical Transactions, vol. lxxiv. p. 142.

This phenomenon is owing to the fire neutralizing the acid.

into that high florid red colour, and oils the fame.

That part of the mercury goes along with the acid, in forming the oxygen gas, hath been fully proved by Dr. Priestley : This is still farther confirmed by nitre. Nitre is that body best adapted to form oxygen gas, an ounce producing half an ounce of oxygen gas. Now, alkaline salts are found to be already fixed and concentrated by nature ; therefore, part of the process is already executed, the fire being already fixed : Consequently, the acid and the alkali only want fire enough to aerilize them.

What then must we think of that hypothesis, which supposes that a strong acid and an alkali can be separated after they are united, and they say, that by a supposed separation, and a supposed attraction, the acid is decompounded of its oxygen gas and phlogisticated air. But where is their proofs that alkalies have any attraction for phlogisticated air ? They have none. Nay, the most whimsical (or shall I call it absurd) part of all is, (as Dr. Harrington has shown, see p. 17. of this Treatise) that Dr. Priestley got such a quantity of airs from nitre ; that in one process the alkali must have attracted the acid's dephlogisticated air, and its phlogisticated air in the other process ; and in both cases, both airs being nearly above the weight of the acid : But enough of such absurdities.

Dr. Harrington says in his Letter, p. 87,  
“ But the most striking, is the acid called fixed  
“ air.

" air. Calcareous earths are well known to  
" chemists to consist of pure fixed air, and the  
" pure calcareous earth; if exposed to fire, they  
" will produce different airs, viz. fixed air;  
" phlogisticated air, common air, dephlogisti-  
" cated, and inflammable air: Just agreeable  
" to this regular gradation, proving that the  
" fire combines with the acid and earth, first  
" expelling the fixed air that is loosely combin-  
" ed with the earth; then the acid that has  
" got a small saturation of fire, and as the fire  
" increases, forming a regular gradation, so as  
" at last to form inflammable air: But, agree-  
" able to their explanation, the fixed air must be  
" formed of all these airs. That the calcareous  
" earth is not impregnated with phlogiston, is  
" proved from its forming pure dephlogisti-  
" cated air with the nitrous acid, and from its  
" being all capable of being formed into phlo-  
" gisticated air with the vegetable acid; here,  
" agreeable to the new aerial proofs, it cannot  
" certainly be supposed to be all pure phlogif-  
" ton. Therefore it surely cannot admit of  
" any other explanation, than the one which  
" our theory give of it." The fixed air just  
runs the same gauntlet with the electric spark,  
producing the same airs. Dr. Priestley likewise  
says, vol. III. p. 89, " When I had recourse to  
" my tall glass-vessels, I used an ounce of salt-  
" petre pounded; and filling the vessel up to  
" the mouth with pounded flint, I took the  
" produce of air at nine times, each about  
" three quarters of an ounce-measure. The  
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" first produce was not quite so good as common air, the second was of the same degree of purity with common air, the third rather worse; but the fourth was so far dephlogisticated, that one measure of it, and two of nitrous air, occupied the space of one-fifth less than one measure. The fifth produce was still better; for one measure of it, and two of nitrous air, occupied the space of half a measure. The ninth was about the same degree of purity; and the rest, I presume, were not much different."

One of the experiments which induced chemists to adopt their present hypothesis, was, that upon the nitrous air being added to pure air, they would produce a fermentation, and decompose each other. Now, to take the oxygen gas, which is formed from nitre, and mix it with nitrous air, which is proved to be an acid air, as the acid predominates in it: For, if it is neutralized as in nitrous dephlogisticated air, so as not to affect the vegetable juices, it will not decompose pure air. That nitrous air is compounded of the nitrous acid, appears clear from the electric spark decomposing 3-4 of it into that acid. Upon its being mixed with the oxygen gas from nitre, the acid will chemically attract the fixed fire or alkaline salt of the oxygen gas, as being a stronger acid than its own; which is aerilized and weakened: In consequence of this, they will produce an effervescence or fermentation, and so much fire will be set loose from both airs, as shall

shall cause them to be decompounded, forming a phlogisticated nitrous acid. That this is really the case, is evident from one of Dr. Priestley's experiments. He says the nitrous vapour will equally decompound pure air. He also says, vol. III. p. 193, "Lastly, I found, in the course of these experiments that the power of this red vapour to phlogistinate common air was much greater, and acted much quicker, than I had imagined when I made the first observation of the kind. For after the former observation, I filled another phial with the red vapour, and immediately afterwards opened it under water; when the water, rushing in, filled about half of it, and the remaining air was found completely phlogisticated, not being in the least affected by nitrous air." The vapour here named is the pure condensable nitrous vapour.

This is Mr. Lavoisier's grand argument; for supposing, that the oxygen gas, with a great part of its caloric, is condensed into the nitrous acid; or when the nitrous and oxygen gas decompound each other, that they will produce little heat comparatively. But the cause is this, in their condensation, the greatest part of their fixed fire or phlogiston is not set loose or free, but is condensed with the airs.

For, according to our theory, we have shown that pure air must necessarily possess both fixed fire and an acid, in order to support combustion. The same argument will take place, when the factitious dephlogisticated air, from the  
calx

calx of metals and the nitrous acid, are decom-pounded by the nitrous air: For the nitrous acid of the nitrous air attracts the fixed fire, which is neutralized or concentrated with the acid, water, and a small part of the earth of the calx, which form oxygen gas; as having a stronger attraction for the fire, than the airs acid have. Dephlogisticated nitrous air, which is a compound of the nitrous acid and phlogiston, more amply confirms this doctrine; for the nitrous air will not decompound it; its acid being equally as strong, being the same acid as that which the nitrous air possesses; i. e. being weakened by no operation, but what the ni-trous acid itself has undergone\*.

Now, I hope, we have clearly shewn by a regular chain of reasoning, and convincing arguments, how, and of what, dephlogisticated air is formed. We shall next examine Mr. Lavoisier's explanation of these phenomena. Agreeable to his theory, all calces of metals should possess dephlogisticated air; and should, upon their reduction, give out their oxygen gas. However, we find no calces do it, but the *mercurius calcinatus per se* and *minium*. Dr. Harrington says, that their giving out dephlo-gisticated air, may be owing to their being formed into calces by so mild a process; and therefore, they imbibe their oxygen gas entire, without decompounding it: For, as the calcin-ation is effected by the heat expelling their phlogiston

\* Dr. Harrington has fully proved, that fire is attracted and fixed by acids and earths. See his Thoughts on Air.

phlogiston entire, and imbibing the oxygen gas; so, no other calces, but these two, will give out oxygen gas, because their calcinations being stronger, require actual combustion.

But here I must beg leave to differ from Dr. Harrington, and suppose, that actual combustion takes place in all the calcinations: For, Mr. Lavoisier found, that in the calcination of mercury, he could not get it to imbibe all the oxygen gas of atmospherical air: The calcination stopped before the air was quite injured: The case is just the same in every combustion. A candle, for instance, will not burn in atmospherical air, till it has totally consumed the air, leaving it in the same state as the calx of mercury. And Dr. Harrington, I dare say, will agree with me, that combustion must turn oxygan gas to an acid; *i. e.* take from it its fixed fire, which neutralized the acid: Besides, the time requisite for both these calces to be exposed to a strong heat, implies, that they are both receiving or concentrating heat at the same time; otherwise they will not give out dephlogisticated air.

Mr. Lavoisier himself found, that most calces can be reduced by a strong mirror, without addition; and Dr. Priestley found, that in their reduction, they do not give out oxygen gas, but only an acid air. Nay, some retain their acid so strong, that the fire will not expel it, though, nevertheless, they will be reduced. And (as Dr. Harrington very properly observes) did their reduction depend upon their being

being separated from their fixed air, or oxygen gas, they should be reduced, as they parted with them. But you may get all their airs from them, and they will still continue calces. I took an ounce of minium, and reduced it with inflammable air; and I accurately noted the quantity it imbibed from its reduction: I then took from the same quantity of minium all its oxygen gas, it producing a considerable quantity; I then reduced this minium in the same inflammable air, and found, that it took an equal quantity of the air for its reduction. Now, Mr. Lavoisier, I should certainly think, must allow this experiment to be decisive against his theory.

Dr. Harrington hath clearly proved the following facts; namely, that the calx of iron, made by steam, is formed by the calx imbibing the pure water; and, that the calx of iron, formed by burning it with the oxygen gas, is from its imbibing the acid contained in the gas: But nothing but pure water is found in the calx made by steam. I have likewise found, that the calx of mercury made in distilled water, by strong agitation, (a mill, for instance, one of Dr. Priestley's experiments) will give out no oxygen gas, but pure water only, at its reduction; and yet Dr. Priestley has proved it to be a calx, viz. the *precipitate per se*. See Priestley, vol. iv. And I have found, that, at its reduction, it imbibes inflammable air, and is a calx similar to that made from iron by water.

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We have proved that metals may be calcined without oxygen gas; and had Mr. Lavoisier only looked into the different recent chemical experiments, he must have seen his hypothesis flatly contradicted. I have found, that if air is admitted in a slow and gradual manner to phosphorus, the phosphorus being slightly moistened, so as to diminish the intensity of its combustion, the deflagration will be so gentle, as not to imbibe the pure air, but only turn it into fixed air: Yet nevertheless, the phosphorus is turned into the phosphoric acid. Now, both these facts are in direct opposition to their theories. For, the pure air ought not to have been changed into fixed air, and the phosphorus should not, without imbibing the pure air, have formed the phosphoric acid. In a word, conformable to Mr. Lavoisier's hypothesis, there ought to have been no combustion or heat, unless the air was imbibed into the body of the phosphorus; for the phosphorus possesses no carbone\*. But what Dr. Harrington says in his Letter, is still more opposite to, and more strongly proves the falseness of Mr. Lavoisier's theory §.

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\* These observations and experiments certainly require an answer, and yet the chemical philosophers, to whom they were addressed, have not yet deigned to give one: Do they not think them worthy of an answer? Or, (which is very probably the reason of their silence) Do they deem them unanswerable?

§ But let us try the phosphoric acid with the calces of metals; by being mixed with minium, Dr. Priestley found, instead of its giving empyreal air, it in reality formed inflammable

In reading the works of the immortal Scheele, who was well grounded in the doctrines of the old chemical school, and perfectly acquainted with its principles; we find a clear proof, that different chemical bodies, by being exposed to heat, become phlogisticated. But he erred by embracing that theory which maintains, that fire is compounded of phlogiston

mable air. He says, vol. IV. p. 136, "In order to try whether this acid had any of the properties of the nitrous acid, I mixed it with some minium out of which all the air had been expelled by heat. This substance, in this state, I had found, when mixed with nitrous acid, yields dephlogisticated air, but no air at all with the vitriolic or the marine acid. The phosphoric acid mixed with this minium with little or no sensible heat, but the mixture exposed to the flame of a candle, yielded air very plentifully, and it was very turbid. I received it in lime water, but it did not precipitate the lime, except in the smallest degree. The air I got in this method was not affected by nitrous air, nor did it affect common air, but was strongly inflammable, burning with a bright white flame; and the smell of the air was the same with that of the strong smell of phosphorus. The yellow minium became of a darkish grey colour, or nearly black by this process."

This must appear inexplicable according to the modern doctrines, for it is well ascertained by experiment, that the phosphoric acid is phosphorus robbed of its inflammable air; for, by uniting it with inflammable air, phosphorus is again formed. But Mr. Lavoifier says, that the phosphoric acid is phosphorus and empyreal air; then, if that was the case, from whence comes the inflammable air when the acid is distilled with the calx of lead? The calx of lead, agreeable to the modern theories, is likewise formed of empyreal air and lead; if the process is pushed with a strong fire, the calx is formed into lead; here there is not only the empyreal air of the phosphorus, but likewise that of the calx lost. And likewise here is a great deal of phlogiston generated; so much phlogiston or inflammable air (without taking to account the phlogiston

ton and pure air; and consequently he supposed, that fire is decompounded in passing thro' bodies, its phlogiston being attracted by the body, and its pure air set loose. However, the fact, that bodies become phlogisticated, is proved by him in its fullest extent.

He also very clearly proves, that the calx of lead and manganese, have the power of dephlogisticating

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phlogiston of the metal) as to form the same quantity of the phosphoric acid and minium which was used in the process, into phosphorus and lead.

This is an experiment which of itself overturns all their theories. According to Mr. Lavoisier's theory, here was nothing to form the inflammable air produced in the process, and the calx when reduced, would part with its empyreal air. Then what comes of it, and what comes of that which the phosphoric acid is supposed to be formed of, according to Mr. Lavoisier? The phosphoric acid should have been decompounded, forming phosphorus, and its empyreal air should have been formed in the process, instead of inflammable air, Mr. Kirwan says the phenomena from the distillation of the calx of lead and acids, are from the fixed air being decompounded; which decomposition forms empyreal air, and reduces the calx: But here there is no empyreal air formed, but inflammable air, and yet the calx is reduced.

Without running into wild theories, there is not a more clear chemical fact, than that the fire is neutralized and concentrated in this process, forming with the phosphoric acid, the inflammable air, which burns like phosphorus (proved by reducing it) and with the metal forming phlogiston. That this is clear, is proved, because the phosphoric acid, when exposed to a strong permanent heat, will form phosphorus; and likewise the calx of lead with a strong fire will be reduced. But it is absurd to say, that in one case the phosphoric acid must part with its supposed empyreal air before it can form phosphorus, and likewise the calx its empyreal air before it can be reduced. For in this process (viz. with the phosphoric acid and minium) there is no empyreal air generated.

Instead

gisticating the mineral acids. Were Mr. Lavoisier to take an accurate and candid review of his experiments, he would see the propriety of giving up his opinion, that the dephlogistication is produced, from imbibing the oxygen gas. For, it is impossible for him to produce this effect, by exposing the mineral acids to pure air ; and yet they should do it, since their attraction

Instead of disputing plain unanswerable facts, we should have supposed it a priori ; for heat or fire must be matter, and that this matter, like other bodies, is capable of uniting with other kinds of matter. There is no way of getting over this, without supposing, as Plato did, that it was a spirit. One of the greatest philosophers, viz. Dr. Franklin, whose judgment and penetration in viewing into nature's operations, are the strongest, formed the opinion of fire uniting with different bodies. He judged from his general observations ; but here we have clear and obvious chemical experiments, which will bear no other explanation. It is an opinion that chemists have always have entertained.

The calces of metals, they say, are formed by the metals imbibing the oxogynous principle, which is, according to Mr. Lavoisier, empyreal air, and according to Mr. Kirwan, fixed air. Though metals are calcined by air, acids, and water, nay sulphur ; yet it is all, they say, through their attracting fixed or empyreal air. But to allow them the strange supposition, that air, acids, and water, contain either empyreal or fixed air they themselves teach that sulphur contains neither of these airs. The immortal Scheele found that, in distilling the calces of metals with sulphur, the calx attracted part of the sulphur's phlogiston, becoming phlogisticated, and that the sulphur was changed in consequence, into a volatile spirit of sulphur ; but that will be said is owing to the sulphur's attracting the oxogynous principle from the calx. Even the calx of iron, formed by the steam of water, will produce these phenomena. I took the different calces of lead, minium, litharge, &c. and after discharging all the air from them, (this supposed oxogynous principle) yet the same volatile spirit was formed, and the calx became phlogisticated.

attraction for it is said to be so great, as to rob the calx of lead of pure air, whose attraction for it is said to be so great. But let Mr. Lavoisier take the mineral acids in their most dephlogisticated state, and pass pure, or their artificial dephlogisticated air through them, which may easily be done in the vessel for saturating water with fixed air, and he will find the air injured, and the acids phlogisticated: But it should have been the reverse.

My readers, I hope, can entertain little doubt, from what has been said, that alkaline salts, oils, and other phlogistic bodies, when compounded, give out an amazing quantity of fire; which clearly shews them to be formed of fixed fire. This being so, we find that acids are the bodies which nature employs to concentrate it, just as the vegetable acid or fixed air, water and earths, are employed in oils, bitumens, wood, &c. and the mineral acids in sulphur, nitre, &c.: Therefore, when these acids are united with earths or water, which (as Dr. Harrington observes) are the great bodies to concentrate fixed fire, we should not be suprised, if fire could be artificially concentrated; particularly by aerilizing these bodies.

phlogisticated. But to make the experiment beyond all cavil, I distilled sulphur and iron with a strong fire; now they all allow, that neither of these bodies contain the least of the oxogynous principle. At the first, as Mr. Scheele observes, the sulphur's expels the iron's phlogiston as inflammable air; but as the fire becomes stronger, the calx of the iron attracts part of the sulphur's phlogiston, which becomes the volatile spirit of sulphur, and the iron becomes phlogisticated. See Harrington's Letter, p. 103.

dies. Dr. Black has fully shewn, that a great quantity of fire is necessary to aerilize even vapours; but we may be certainly assured, that a greater quantity of heat is required for the formation of permanent airs or vapours; or to form a fire air, compounded of an acid, fixed fire, and water: But in forming the salt air, if I may be allowed the term for oxygen gas, a little earth may be necessary.

Many reasons may be alledged to shew that it is very probable that they form a body some thing similar to nitre. Nitre forms the largest quantity of oxygen gas; and, like oxygen gas, it makes combustible bodies burn in the same manner. This is strikingly proved in the burning of nitre and charcoal, or of any other combustible body. They burn equally, as if they were burning in oxygen gas. Moreover, it is evident that alkali has much to do in the process; for, if the acid is united to the charcoal without the alkali, they will not burn, and the alkali is consumed in the process: Therefore this is a conclusive proof that charcoal, &c. will not burn, but with an acid and an alkali, or fixed fire. And as oxygen gas equally promotes the combustion, we may suppose that it is formed of similar bodies as nitre, viz. an acid, an alkali, and water: This Dr. Harrington has long ago observed to be the composition of oxygen gas.

And if, (as Dr. Priestley and others have found) in the solutions of metallic earths and acids, by being exposed to a strong heat, long continued,

continued, the earths would be deposited with saline qualities and properties; then, when these bodies were aerilized, they would shew more of these qualities, from the greater quantity of fixed fire necessary to aerilize them. This is strikingly seen in the rust of iron, which is formed of the acid of the atmosphere. It will, by being forced in a still, take the form of vapour, and receive so much fixed fire as to form the volatile alkali. The rust of iron, says Mr. Keir, by being distilled, forms the volatile alkali. This explains the reason why many chemists have found the volatile alkali in their experiments, when they least expected it, without having recourse to those wonderful transmutations they speak of. For, as Scheele and other chemists found that oils, salts, and other bodies become volatile by receiving a larger quantity of fire; so alkaline salts are the same, upon the same principle: For as salts are formed from fixed fire; fixed by acid, earths, and water; so a larger quantity of fire thrown in will make them become volatile.

The acetite of potash, as Mr. Lavoisier calls it, is a neutral salt formed of the acetous acid and the vegetable alkali. Now, it is well known, that this salt (as Mr. Lavoisier observes, page 270.) will give out ammoniac in distillation; and by ammoniac he means the volatile alkali. Then, can there be a stronger proof of the truth of our hypothesis, that an acid, when united to other bodies, as salts,

salts, earths, &c. concentrates a great quantity of fire, so as to change the fixed to the volatile alkali, as in this process of the acetite of potash? And it also clearly ascertains this fact; that by an addition of fixed fire, the fixed alkali will become volatile. And it proves, which is a fact of still greater consequence, that when an acid and an alkali are exposed to heat, they will concentrate a very great quantity of fire: Therefore the nitrous acid, which is an acid of a stronger attraction for the alkali, and for fixed fire, will, (as we have shewn) in the distillation of nitre, attract and concentrate a quantity of fire, and form oxygen gas. All these are fair and clear deductions from unquestionable facts.

With this review we may be able to account for all the phenomena of the late numerous experiments made by chemists for these twenty years; and I aver, that there is not one of these phenomena, but may receive an easy and rational explanation from this hypothesis. When oxygen gas is formed from acids and earths, we have supposed that the acid, with fixed fire, and a little of the earth, are aerilized into the neutral aerial salt or nitre, called oxygen gas. That there is a small quantity of earth, as much as is necessary to combine the fixed fire, the acid and water together, is very probable: For (as Mr. Beaume observes) it has all along been supposed, by old chemists, to be a kind of basis to salts; and these bodies, I should think, form both oils and salts. That all bodies,

bodies, which contain a great quantity of fixed fire, are formed of acids, water, and earths, is what Dr. Harrington has always taught, nay I will venture to say, proved. But he proves that the pure air of the atmosphere is not formed of earth or the strong mineral acids, but of water and a weak acid, called the aerial acid or fixed air; and therefore an air very different from the factitious oxygen gas: But that the gas may contain a little earth, when made from metals, is very probable. Dr. Harrington says in his Letter, p. 135, "As our theory supposes that there is a small quantity of earth in the artificial empyreal air, to ascertain this, I examined very accurately the residuum, after decomounding the empyreal air by the electric fluid; and I always found a deposition of an earthy sediment. That the quicksilver could not produce it, I ascertained, by putting in a column of distilled water between it and the air; such a quantity as would dilute the acid, so as that it could not in the least act upon the mercury. Besides, if the process is conducted over soap-lees or lime water, by a chemical examination, you will equally find that there has been an earthy deposition from the air's being decompounded." And Dr. Priestley has brought a number of experiments to prove, that nitrous air contains a little earth, and how difficult it is to detect it. Besides, as Dr. Harrington rationally supposes, water is earth, with a great quantity of latent heat.

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heat. Ice, by having a greater quantity of heat thrown into it, becomes water; so earth, by having a greater quantity of heat thrown into it, may become water. Therefore, he farther supposes, that by the earth's imbibing a quantity of fixed fire, so as to be aerilized, as in oxygen gas, it then has the properties of water.

Dr. Harrington has given an indisputable fact to prove that fixed air, water, and the fire of the sun, produce pure air; and I wonder how chemists can overlook such obvious experiments. He says in his Letter, p. 116,  
" But after analyzing the air, we will likewise  
" prove the synthetic part. I took the fixed air  
" I got from the magnesia, and exposing it to  
" heat and moisture, formed it into pure air  
" again. See my Thoughts on Air, p. 307."  
" But we have another experiment in making  
" this artificial air, which will shew us the pro-  
" cess in a more clear way, so that we cannot  
" positively mistake it; which is, making it  
" from the fixed air and the purest water. In  
" impregnating water with fixed air, and ex-  
" posing it to the sun, the fixed air will be  
" formed into empyreal air. Agreeable to our  
" theory, the sun, fixed air, and water, satu-  
" rate themselves into this compound empy-  
" real air. But agreeable to their theory, the  
" fixed air is a compound, and must, in this  
" process, have been decompounded; that is,  
" each ounce of fixed air must have been de-  
" compounded into three ounces of inflam-  
" mable air, and one of dephlogisticated air.  
" As

" As this fixed air then, is compounded of so  
" much phlogiston, we must certainly observe  
" it upon the decomposition. I took a gallon  
" of the purest distilled water, impregnated  
" with fixed air, and exposed it to the sun. I  
" took another gallon of the same water, and ex-  
" posed it equally to the sun. Upon examining  
" them after exposure, the water impregnated  
" with fixed air, yielded me a large quantity of  
" empyreal air; but the pure water none.  
" Then I endeavoured to find the quantity of  
" inflammable air that must have been set  
" loose from the fixed air, but there was not a  
" particle to be found in it: Nor was it depo-  
" sited as phlogiston; for if we are to retain  
" one old chemical fact, phlogiston and water  
" repel each other; therefore, we should have  
" found it swimming upon the water's surface:  
" But in short, the most sceptic ingenuity could  
" not find the least of it any way. Upon ex-  
" amining the other gallon of water, there was  
" a greater appearance of phlogiston in it.  
" But undoubtedly it is only to be accounted  
" for in this way, the sun's rays united with  
" the fixed air and water, and formed this em-  
" pyreal air; for in the other water, as it had  
" no fixed air to saturate the rays, they united  
" themselves in some measure to the water,  
" and very slightly phlogisticated it; and like-  
" wise a thermometer that was in the impreg-  
" nated water, shewed a little less heat than  
" the other water, owing no doubt to the sun's  
" rays being absorbed; also, there was a little

" more loss of the impregnated water than the  
" other, owing to its being absorbed into the  
" compound empyreal air. That it does possess  
" water, as does likewise inflammable air, ap-  
" pears from this, that when they are fired to-  
" gether they turn to water."

When water robs iron of its phlogiston, it forms an oily scum upon it, which is very considerable; therefore, in this experiment of the fixed air, being formed into dephlogisticated air, there ought to be the same scum.

Now, can Mr. Lavoisier, upon the formation of pure air, from fixed air, find the carbone, which ought to have been deposited in the water, being set free from its combination with fixed air? Nay, will it not give our reader a laughable surprise, when I tell him, that Mr. Lavoisier seriously proposes a manufactory to obtain charcoal by the decomposition of fixed air. See his Elements, page 230. But I will hint to him a better manufactory, and one more conformable to his hypothesis. He says, that water consists of hydrogen and oxygen gasses; and, that these gasses, with the addition of *carbone* or charcoal, form alkohol or spirits. Now, as the river *Seine* produces plenty of water, and as charcoal is a cheap commodity, the transmutation of water into spirits would be a manufactory that would turn to good account. This would lower the price of French brandy in Old England; or, as several of our English chemists are no less industrious and ingenious than Mr. Lavoisier, the Thames might

might be turned into good British spirits; which would render that article still cheaper: But alas! this, I am afraid, will still be one of the chemical dissiderata: And, as this kind of chemistry will not effect so much good; an alarm may be spread on the other hand. For, according to them, water is formed of inflammable and oxygen gasses, two bodies the most combustible in nature. If therefore they should be able to set the Thames on fire, London would be in danger of being reduced to ashes.

The very singular hypothesis of water being formed of inflammable and dephlogisticated air, is strenuously supported by modern chemists; as, upon this, their new chemistry depends. Mr. Lavoisier endeavours to support this hypothesis; for he says, that by the decomposition of water, by steam passing through charcoal, the water is decomposed, the hydrogen gas being separated from the oxygen gas: But, were his hypothesis true, this inflammable air or hydrogen gas, instead of changing oxygen gas into fixed air, should turn it into water, which it does not. Nothing more, I think, needs to be said, since Dr. Harrington has fully explained, how the inflammable air is formed in this process; which is a direct confirmation of his doctrine, that fixed fire is phlogiston \*.

## Flame

\* The theory is simply this. Vegetable bodies are principally formed of the vegetable acid, with water and an earthy basis; which is nature's own compound to attract the fire

Flame is very delicate. It will be extinguished in air that will still support animal and vegetable life. We have, I hope, satisfactorily proved, that the life of flame exists by setting loose fixed fire; and also, that both an acid and fire are requisite to its support. *Therefore it is a natural conclusion, that the atmosphere, unless nature had formed it of both an acid and fixed fire, would not have been proper for the support of flame.* And what more fully confirms this is, that the rich factitious dephlogisticated airs, (dephlogisticated nitrous air for instance) which possess a strong acid, will support flame with the greatest avidity, owing to the

fire of the sun concentrating it. When it is exposed to the fire, the fire is concentrated or attracted into the compound, forming charcoal; and when the heat is pushed further, there is such a high concentration, as to form inflammable air; but, if an over proportion of water enters into the compound, a considerable less quantity of fire enters, and they form fixed air.

This is by no means singular; all compounds, agreeable to the greater or less proportion of the different ingredients, form different bodies, seen clearly in the acid airs. Thus, I have proved, that phlogiston and the nitrous acid form nitrous air; but by adding a greater proportion of phlogiston to the air already generated, or before its generation, it will produce either inflammable or empyreal nitrous air. The same happens with regard to the vitriolic acid air; a greater proportion of phlogiston, forms it into the hepatic air. In the marine acid air, it is the same; by exposing it to iron, it will reduce the iron into a calx, seizing upon its phlogiston, and forming inflammable air; which is just the same process as the nitrous acid air, in forming inflammable or empyreal nitrous air with iron. But, from the singular theories of airs, they will have the latter to be a different process. See Harrington's Letter, p. 26.

the strong nitrous acid in it; but it will not do for the support of either animal or vegetable life. As a farther confirmation of this, the fixed alkali and sulphur will not enflame together; nor will phlogiston, or the spirits of wine, support flame without the aid of an acid, dephlogisticated nitrous air, for instance.

Dephlogisticated air is supposed to be an element: But, can chemists really suppose, that the pure part of atmospherical air, and the factitious dephlogisticated airs, particularly the dephlogisticated nitrous air, are the same; which, conformable to their hypothesis, they ought to be? Nay, the last supports flame with the greatest avidity, but is as noxious to animal and vegetable life, as the most mephitic air we know of. Its effect in depriving an animal of life is the very same, as that of the most noxious phlogisticated air. Examine them chemically, and there will be found the same differences in all the oxygen gasses; by weight the same; in short, the same differences will appear in every comparison.

We have already observed, and it is conformable to our hypothesis, that the atmospherical air is a body similar to nitre, which is formed of an acid, and an alkali, (which is fixed fire and water.) Atmospherical air, or the pure part of it, is formed of an acid, fixed fire, and water: This similarity is strikingly seen in combustion. Combustible bodies will not burn without the aid of atmospherical air, or nitre; and the nitre and the air are left, after

after the combustion, in the same state, both having lost the fixed fire that neutralized them. The alkali has disappeared, and also the alkaline body which neutralized the air's acid; it being left in the state of fixed air, or an acid. And, that this body which aerilized both the nitre's acid, and the air's acid, is necessary to combustion, is a certain truth; for, if you attempt to burn combustible bodies in these acids, they will not burn. And, as we before observed, this nitre is the body which will produce the most of factitious dephlogisticated air. Dr. Priestley formed above half of the nitre into dephlogisticated air; and by adding more nitrous acid, you may still go on forming more air. That the alkali disappears, making a part of the air, is certain, from the quantity formed: That it cannot be formed, either from the acid or the water, we have fully proved, see page 15. And Mr. Watt, from Dr. Priestley's experiments, examined the acid, and found such a quantity of it had come over entire, that he says, it was the greatest part of the acid employed in the process.

My reader is, I hope, satisfied by what has been said, that fixed fire forms phlogiston, alkaline salts, &c. That those bodies best adapted to neutralize fixed fire, are acids and water, or earths; and, that atmospherical air is formed of a mild acid, a mild concentration of fire, and water. But, as we can make a factitious gas that will perform the offices of air

air, we shall endeavour to investigate it: For, according to our theory, the air, necessary to combustion, must be formed of an acid, and fixed fire or phlogiston. The acid is necessary in combustion to assist the fixed fire in being let loose, or breaking from its bond of union, into a free or actual state.

We shall take the nitrous acid, which, from its attraction for fire, seems to be one of the best acids, and unite it with concentrated fire or phlogiston.

By exposing the nitrous acid to the essential oils, spirits of wine, the phlogiston of metals, we may form an air much better adapted to the purposes of combustion, than atmospherical air; because it is formed of a stronger acid, and of a richer or stronger phlogiston, or concentrated fire. Chemists call this air, *dephlogisticated* nitrous air; an error which proceeds from their having adopted very absurd chemical theories concerning air: But that it is a phlogisticated air, formed of the bodies we have named, I shall, (agreeable to my former promise) prove in the most satisfactory manner: And, I will venture to say, that those who disallow it, from the power of prejudice, deserve not the name of chemists \*.

It appears to be just, even at the first glance, of what has already been said: For, will not

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\* But in the name of wonder! is not the nitrous ether formed of the nitrous acid and phlogiston? And is it not made in the same manner and of the same bodies as the nitrous *dephlogisticated* air?

the nitrous acid, and phlogistic bodies, essential oils, &c. burn in their condensed state? And, will our modern aerial chemists deny this to be a phlogistic composition? Experiments have always been the great and sure path to walk in, when we investigate the phenomena of nature; but experiments without much circumspection and knowledge, are apt to lead philosophers into error: This we shall prove in this treatise, in the most striking manner, and shew, how our modern theorists have been led into such errors by their experiments.

Essential oils and the volatile alkali are the most volatile bodies in nature. These, when united with the nitrous acid, the attraction of which for the oils is so strong and rapid, produce, in their union, very great heat. Now, that these bodies, when united, should not attract each other, nor should be aerilized together; but that the volatile oils and the volatile alkali should so far counteract their volatility, as to become fixed, when exposed to the strongest fire; should so decompound the acid, as to leave no attraction for it in its compound state; nay, to decompound it into three distinct bodies; viz. dephlogisticated air, nitrous air, and phlogisticated air: I say, all this is such an absurdity, as not to be reconciled to common sense. But, as one error imperceptibly leads to another, so is one absurdity the cause of another; mortification is the consequence, which might have been prevented, had

had chemical philosophers but paid a proper attention to Dr. Harrington's theory: And certainly, not to have attended to what he has said, is highly culpable: I shall give it in a long quotation.

There is no end of their errors; for, is it not evident, that if an alkaline air is fired in this nitrous dephlogisticated air, an acid and water will be in the residuum? the phlogiston of the air and the alkali have disappeared, going off as heat or fire in the combustion. This, our aerial philosophers deny. The alkali must have entered the dephlogisticated air, and the latent heat only must have been set loose. But, wonderful! this dephlogisticated nitrous air, shews no test of acidity, no not even to the vegetable juices. The nitrous air, which shews a strong acidity by the vegetable juices, by adding phlogistic bodies to it, to neutralize it, will form dephlogisticated nitrous air; and if alkaline air is fired in this wonderful dephlogisticated nitrous air, it will become the nitrous acid; which, they say, is from having attracted this alkaline air: Such is their hypothesis. This is neither a lame, nor false representation; for, (as Doctors Harrington and Priestley found) if this alkali is fired in their common dephlogisticated air from nitre, they will produce the nitrous acid. It may then be asked, what is chemistry indebted to our modern aerial teachers? Alkalies do not neutralize acids, but form bodies into the strong mineral acids; bodies, which shew no signs of

acidity ; they having been acids, but having had their acidity neutralized with phlogiston : Nay, they blunder from one absurdity to another. Dr. Priestley sometimes calls phlogiston the principle of alkalinity, and sometimes he makes it the principle of acidity. In the Phil. Transl. vol. lxxviii. p. 156, he says, ‘ As there is something in dephlogisticated air that seems to be the principle of *universal acidity*, so I am still inclined to think, as I observed in my last Volume of Experiments, that phlogiston is the principle of *alkalinity*, if such a term may be used ; especially as alkaline air may be converted into inflammable air.’ In vol. VI. p. 99, where speaking of vapour operating upon charcoal, and producing fixed air, he supposes, that inflammable air and water produce fixed air.

Let us see how our hypothesis agrees with this phenomenon. But it is so obvious, that I should apologize to my reader for explaining it. The dephlogisticated nitrous air, when richly made, having fire applied to it, penetrates its own phlogiston, which kindles such a quantity of fire, as thereby to set loose all the fixed fire which formed these bodies, alkaline air, &c. leaving the acid and water in the residuum, with a little aeriform matter, formed of the acid, and some phlogiston not totally set loose.

But I shall give a long quotation from Dr. Harrington’s Thoughts on Air, published in 1785. And, I think, my reader will be much surprised, that his principles, and the arguments which support them, have not been more

more attended to;—a plain proof that the true solid principles of chemistry have been kicked out of doors, to make room for the *aerial flights* of modern chemists. He says, p. 56, ‘Dr. Priestley has made this kind of air by several processes; adding a larger quantity of phlogiston to nitrous air. It seems remarkable, that Dr. Priestley should suppose it was nitrous air dephlogisticated. Vol. II. p. 121. The Doctor says,’ ‘At Mr. Lavoisier’s I saw, with great astonishment, the rapid production of, I believe, near two gallons of air from a mixture of spirit of nitre and spirit of wine heated with a pan of charcoal; and when that ingenious philosopher drew this air out of the receiver with a pump, and applied the flame of a candle to the orifice of the tube through which it was conveyed into the open air, it burned with a blue flame; and working the pump pretty vigorously, he made the streams of the blue flame extend to a considerable distance.’

‘He was struck with the experiment, and repeated it when he came into England, using one-third of the spirit of nitre to two of the spirit of caine; but this experiment does not seem to have been so remarkable. His air not being so inflammable, nor burning with that enlarged flame\*.

‘Dr.

\* The Doctor says, vol. II. p. 122. “My first idea was, that this air was the same thing with the phlogisticated nitrous air, which I had procured by exposing pieces of iron or liver

' Dr. Priestley calls these airs dephlogisticated, from their resembling dephlogisticated respirable air, as they admit a candle to burn with that extended flame, and in a brisk vigorous manner.'

' It appears to me very singular that this vapour should be dephlogisticated; but let us examine it. The Doctor used two-thirds of spirit of wine, and one of the spirit of nitre, and formed them into vapour by the flame of a candle. But this heat was, by no means, equal to Mr. Lavoisier's; his heat being from hot charcoal. This vapour, though not so inflammable as Mr. Lavoisier's, consisted of these two bodies, two parts of alcohol and one of spirit of nitre. The inflammability of alcohol I need not mention, being well known to chemists; as to the spirit of nitre, the Doctor thinks, it contains so large a proportion of phlogiston, as to be of itself capable of phlogisticating common air. These two bodies likewise receive a large quantity of heat from the hot charcoal in the course of the process, as we have shewn that bodies require so much to form them into vapour; how then can we call this vapour dephlogisticated? It is by the same proportion of ingredients, and by much the same process, that chemists make nitrous æther. The vitriolic æther, which

liver of sulphur to nitrous air, the phlogiston of the spirit of wine being, as I supposed, disengaged in this process, and becoming incorporated with the nitrous acid, in the same manner as the phlogiston that is disengaged from the two other substances."

‘ which is only the vitriolic acid used instead of the nitrous, is so inflammable, that if two drops of it be put into two quarts of common air, and set fire to, it will make the whole explode with a loud report, phlogisticating, as Dr. Priestley calls it, the remaining air.

‘ In vol. II. p. 130. The Doctor formed a vapour of the spirit of nitre and oil of turpentine, and in order to judge, if an acid prevailed in this air, (which he calls dephlogisticated) he mixed it with alkaline air; they instantly formed a white cloud, making the nitrous ammoniac as he supposed; only half of the vapour united with the alkaline air, the other half, upon applying a candle, made a considerable explosion. This experiment scarce requires a comment. That part of the acid, which was not saturated with the oil of turpentine, formed an union with the alkali, and then the other strongly saturated with the oil of turpentine, being left more at liberty, exploded \*. But the Doctor says, there was not quite so great an explosion as I have observed to have been made by a quantity of nitrous air, of what he now supposes dephlogisticated.’ ‘ Vol. I. p. 217. The Doctor

\* Vol. II. p. 125. he says, “ Considering this flame with attention, I thought it very much resembled that which is produced by a mixture of about one-third inflammable air, and two-thirds nitrous air; and concluded, that it was probably composed of them both; the nitrous acid forming nitrous air, by seizing upon the phlogiston of spirit of wine; and there being a redundancy of inflammable matter, sufficient to render the air partially inflammable.”

' tor at this time formed an hypothesis of the  
' vapours being phlogisticated, though in his  
' latter publication he has adopted the idea of  
' its being a dephlogisticated vapour †.

' If we should mix the two ingredients in a  
' vessel, the spirit of wine, or the oil of turpen-  
' tine, and the spirit of nitre, would chemists  
' call it a dephlogisticated mixture? certainly  
' not; then certainly, with far less propriety it  
' may be called a dephlogistic vapour, after it  
' has imbibed so much of the fire or heat from  
' the charcoal or candle, as to make it a per-  
' manent vapour. In arguing in this manner,  
' we can appeal more immediately to our senses,  
' and determine with a greater degree of cer-  
' tainty and satisfaction †.

' The three metallic bodies which the hon.  
' Mr. Cavendish found to produce inflammable  
' air with the vitriolic and marine acids, viz.  
' iron, zinc, and tin, with the nitrous acid, pro-  
duce

‡ Now, agreeable to the Doctor's present hypothesis, the alkaline air must have united with the oil of turpentine, and formed the nitrous ammoniac; and the nitrous acid must have been left at liberty to have admitted a candle to burn in it as dephlogisticated air. When I repeated the process I found, that if I made use of strong oil of turpentine, and mixed the air with the alkaline, it would explode; but when I used weak oil of turpentine, it only burnt like dephlogisticated air, or what I call empyreal air.

† What an idea! that the pure nitrous vapour should be supposed to be so highly phlogisticated, which they suppose, from its injuring atmospheric air; while the same acid, formed into vapour with the addition of twice the quantity of spirit of wine, and this to be called a pure dephlogisticated vapour, is certainly wonderful.

duce this peculiar kind of air. Sometimes it will explode, sometimes burn directly like empyreal air, admitting a candle to burn in it with great vehemence, and a bright extended lively flame, with a crackling noise, and sometimes with only a bright natural flame; all these varieties Dr. Priestley found in it\*.

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\* Here is a question that, particularly forces itself upon our judgment; how comes it, if it is a dephlogisticated nitrous air or vapour, that the same bodies, which yield with the most acids, viz. the vitriolic, phosphoric, marine, and vegetable acids, inflammable air, should, with the nitrous, yield a dephlogisticated nitrous air or vapour? When the other metals which produce strong nitrous air, (which Dr. Priestley supposes to possess so much phlogiston, equal to inflammable air) with the above acids, vitriolic, &c. will produce no inflammable air. And those metals which produce this dephlogisticated nitrous vapour, as the Doctor calls it, will even produce inflammable air, by themselves, exposed to heat. Certainly there is an obvious deficiency in this theory; for this fact alone implies an error in it. Nay, the inflammable air that they yield with the acids, is more than the dephlogisticated vapour with the nitrous acid.

But to be more particular, I took two ounces of zinc, and exposed it to the vitriolic acid, and collected all the air that I could get from it with the assistance of heat, which was strong inflammable air. I exposed other two ounces of it to the nitrous acid under the same circumstances, and collected the air I got from it, which was this peculiar nitrous air. I argued, (*a priori*) that if the zinc that was exposed to the vitriolic acid, which undoubtedly had given out its phlogiston, would be highly dephlogisticated; then that which had given out the dephlogisticated nitrous air would, in course, (if the Doctor's theory be true) be highly phlogisticated. Upon putting the residuum of each of these experiments into a retort, after mixing them with the nitrous acid, I got purer air, that is, more free of phlogiston, from that which had yielded the dephlogisticated nitrous vapour, than from that which had yielded the inflammable air. And this is a test agreeable to the Doctor's own experiments and reasoning, which is incontrovertible.

' He procured this inflammable nitrous air  
' by exposing nitrous air to iron filings and  
' brimstone, in a degree of heat that would  
' make the mixture give out its inflammable  
' air; and which the nitrous air imbibed.  
' But if the mixture is not in that state of dis-  
' charging its phlogiston, it will decompose  
' the nitrous air in the same manner as re-  
' spirable air.

' I have myself repeated these experiments,  
' and am perfectly clear, that if it is new made,  
' and put in a cool place, it will decompose  
' the nitrous air; but if it be put into a warm  
' situation, and has been made some time, it  
' will produce this inflammable nitrous air.  
' In the first process it has just the same opera-  
' tion upon the nitrous air as the nitrous acid  
' has in imbibing its phlogiston.

' Another process in which the Doctor got  
' this air was, by exposing nitrous air to iron;  
' which is performed in this way. The acid  
' predominates in the common nitrous air,  
' which is discovered by its turning the juice  
' of turnsole red, and upon admitting the least  
' common air to it, giving that rough astrin-  
' gent acidity to water, which acid, when the  
' nitrous air is applied to iron, corrodes it,  
' forms into a calx, and is saturated with the  
' phlogiston \*.

' I once

\* Nothing can be more obvious than this experiment.  
The iron is formed into a calx by the acid, and saturates it-  
self with its phlogiston; for how other can we account for  
it?

' I once had an opportunity of seeing a process of this kind go on very distinctly. After making some nitrous air, I filled up the phial with water, which had been weakly acidulated with the nitrous acid. The next morning, throwing out the water, I added to it some spirit of nitre; and after the phial was full of nitrous air, I observed an evident appearance of more air being generated, and it nevertheless did not increase the bulk of that already generated. It must have been the phlogiston of the metal discharging itself, and which would be absorbed by the superabundant acid of the nitrous air. Dr. Priestley has given a similar experiment, and with the direct same result in every circumstance.

' The process by which the Doctor procured the greatest quantity of it, was, by putting iron to a solution of copper in the nitrous acid, he expected to have procured nitrous air, but he procured in great abundance this kind of air; which bears an easy explanation. In dissolving iron in the nitrous acid, the Doctor procured first the nitrous air; but when no more would come, on applying a candle, he procured this air.

' I observed before, that the vitriolic acid, which is not so volatile with iron, produces inflammable air; therefore this part of the

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' nitrous

it? Agreeable to Dr. Priestley's theory of the air's being dephlogisticated, what has become of the phlogiston of the iron upon its being reduced to a calx, and the phlogiston of the nitrous air upon its being dephlogisticated? nothing can be more obvious.

' nitrous acid, which was not evaporized so  
' soon, but was attracted by the phlogiston,  
' and there fully saturated itself with it in a  
' slow and gradual manner, not having a rapid  
' generation of heat to discharge it immedi-  
' ately. Dr. Priestley observed he got more  
' nitrous air by a quick effervescence than a  
' slow one; a quick effervescence generates a  
' stronger heat, and therefore volatilizes more  
' of the acid. Now, we shall not find this to be  
' the case here. The acid being attracted by  
' the iron, but being already united to the  
' copper, leaves it very slowly, and being a  
' weak solution with a large quantity of wa-  
' ter in it; so that as the heat is generated it  
' will help to depress it; the acid, therefore,  
' will act slowly and gradually upon the iron,  
' and by that means it will not form air till it  
' is highly saturated with phlogiston; it being,  
' as Dr. Priestley observes, produced in a gra-  
' dual slow manner \*.

' After what has been said, it may appear a  
' little surprising, that the learned Doctor  
' should adopt the hypothesis of its being a  
' dephlogisticated vapour. But its great re-  
' semblance to empyreal respirable air might  
' incline him to embrace that opinion; and al-  
' so, because the flame of a candle was support-  
' ed in the same bright manner as empyreal air  
' does. To have adopted the contrary opinion  
' would

\* As iron contains more phlogiston than copper; there-  
fore it is the surplus, which remains after reduc ing the cop-  
per, which forms this air; otherwise what comes of it?

would have shaken the great hypothesis, which, not only Dr. Priestley, but most philosophers, have espoused. There is no doubt but animal, vegetable, and the life of flame, are supported by the very same principles in respirable air; therefore, to give up the life of flame, would be to give up the whole. Let us, however, more maturely consider the foundation of this doctrine.

Dr. Priestley, agreeable to his hypothesis, supposed that this particular nitrous air possessed a nitrous vapour, which attracted the phlogiston, which he imagined was produced by the flame, and by that means supported it. But, upon examination, it appears just the reverse; for we have proved this nitrous vapour in the same degree that it is neutralized supports the flame with more vigour; and as it will be found in the Doctor's experiments, which he himself acknowledges, (see vol. III. p. 143.) that when it is in its highest and most perfect state, it then shews not the least sign of an acid vapour, neither in its burning nor by the nicest test that chemistry is master of, by the juice of turnsole \*. And in the experiment mentioned p. 58. when the alkaline air neutralized the nitrous vapour, so as to form the nitrous ammoniac, that it then became so inflammable as to explode with a great

\* And this test, the Doctor himself and all chemists allow, is the nicest and most certain; for by it, the impregnations of fixed air in water we can always detect, and certainly it is an acid considerably weaker than the nitrous.

' great report. And certainly as the strongest attraction exists between alkalies and acids, if dephlogisticated nitrous air was formed of a nitrous acid vapour, that alkaline air would unite with it.

' Dr. Priestley, in his supplemental observations, mentions a fact in support of his hypothesis. I take this opportunity of remarking, that as my motive is to investigate truth, I would not be thought so disingenuous as to suppress any experiment that may seem to militate against my own opinion. No doubt it would have given me a degree of confidence to have coincided with so ingenious a philosopher; but as truth ought to be the object of every one's enquiry, and as the Doctor's liberality of sentiment is so well known, I shall make no farther apology for the present investigation.

' The fact I allude to is, that nitrous air exposed to the scales of iron, which Dr. Priestley supposes a partial calx, produces this air; but if it is not wholly so, if there is still a part of it metal, it will be sufficient to give it phlogiston; and why does not the perfect calx itself form this air? And how comes the iron to be reduced to a calx by forming this air? And besides, if the Doctor had exposed these scales of iron to diluted oil of vitriol, he would have obtained inflammable air.

' The Doctor says, vol. V. p. 344. in the summary view of his principal facts, ' It is procured from iron filings and brimstone, by nitrous

' nitrous air, before it becomes phlogisticated air.' ' This observation of the Doctor's shews that, when the air is perfectly phlogisticated, its acid being neutralized, then it cannot produce this air, having no acid to unite with the inflammable air which escapes from the mixture of iron and brimstone. This mixture newly made, as I observed before, will equally decompose nitrous and atmospheric air; but if it is in a state of giving out its inflammable air, it will make both these kinds of air inflammable. (See Priestley.)

' Dr. Priestley found that this air would sometimes injure common air, and sometimes not; he could not say positively whether it would or not. But his experiments evince this, that when it burns with a blue nitrous flame, then it would affect common air; but when it did not, it would not have the least action upon it \*. We shall find in the course of this treatise, that it is the acid that injures respirable air; therefore, whenever it is fully neutralized with phlogiston, it in course has no effect.

' In demonstration of this, I took a quantity of this air in which the acid predominated, and exposed it to common air, and it injured it. There was a decomposition. Then I took some of the same air from the same process, and exposed it to iron filings and brimstone ' in

\* Its burning with this blue nitrous flame is a direct indication of the nitrous acid; for by adding the nitrous vapour to any inflammable air it will make it burn in this manner.

' in a heat that would discharge their inflammable air ; and after a little exposure I found the air had lost the quality of injuring common air. After this impregnation, I took part of it, and mixed it with some of the nitrous vapour made from the pure nitrous acid, and it regained its power of injuring respirable air. I have done this alternately many times together ; for when nitrous air has lost its power of turning the juice of turnsole red, it will have lost its power of injuring common air.

' But before the iron filings and brimstone give out their inflammable air, if they have so far absorbed the nitrous air as to have imbibed all its acid, leaving a residuum, which will neither shew the principles of this nitrous air nor common nitrous air ; (the properties of which we shall elsewhere explain) then if they give out their inflammable phlogistic matter, there is no acid to unite with it ; therefore the nitrous air will not form this species of air. But Dr. Priestley has given us a very accurate description of the formation of this air, by which we shall be able to judge of its analysis. Exposing the nitrous air to a mixture of iron filings and brimstone over some pure water, he found that the acid was seized on in this process, and not the phlogiston of the nitrous air that was exposed to this mixture, in making this particular species of air in which a candle will burn. But we shall give the Doctor's own words, vol. III.

' page

'page 142 \*.' 'I shall now proceed to note other phenomena attending this process, which the philosophical reader will easily perceive, may be of great use in the analysis of this species of air, as well as help to explain the process itself.' '(The process which the Doctor means, is exposing of the nitrous air to a mixture of iron filings and brimstone, and the air becoming what he calls dephlogisticated.)' 'In order to determine whether the acid, or *phlogiston*, of the nitrous air had been seized upon in this process, I made it over a quantity of very pure water; thinking that, if it should acquire any acidity, it would shew that the phlogiston only had been seized; but that if it should not have become acid, it would appear that the decomposition had been effected by the acid having been seized, and the phlogiston left: And the result seems to determine this question in favour of the latter supposition. For, when the process was over, I examined the water with the greatest attention, and found not the least appearance of acidity in it. It did not even turn the juice of turnsole red; and I do not know a more accurate test of small degrees of acidity than this. On the other hand, in the decomposition of nitrous air by nitrous acid, the water

T ' over

\* With Dr. Priestley's explanation of this process, that the acid is seized on by the mixture, and the phlogiston left; how then can he possibly call it dephlogisticated air, when his opinion is, that even nitrous air possesses so large a quantity of phlogiston? certainly then it must possess more in proportion, when it has lost its acid, and retains its phlogiston.

' over which the diminution was made, becomes much more acid than can be accounted for by the evaporation of the acid introduced into it\*.'

' This perfectly agrees with our hypothesis that the phlogiston emitted from this mixture and from iron, &c. seizes upon the acid of the nitrous air, neutralizing it so, that it could not acidulate the water, and making this peculiar nitrous air, in which a candle burns some-

' candle

\* But we shall relate another experiment of the Doctor's, which equally proves the same. He says, vol. III. p. 146, "That *liver of sulphur* also decomposes nitrous air, by seizing upon its acid, seems to be proved by the following experiment. I put some pieces of liver of sulphur to two quantities of nitrous air confined by quicksilver, and I observed that, in about ten hours, between one third and one half of the air in each of them was absorbed. The next day I admitted water to one of them, but no part of the air was absorbed and after it had passed several times through water, a candle burned in it with an enlarged flame, crackling very much. Also a candle burned in the very same manner in the air contained in the other vessel, which I had not made to pass through water. In this experiment I took particular notice that there was no appearance of the mercury having been corroded, or having been in any other respect visibly affected during the process; as it would have been if every thing acid in the nitrous air had not been united to the liver of sulphur. Perhaps the reason why the air in this experiment bore the passage through water without losing its property of admitting a candle to burn in it was, that, by standing a long time out of water, the constituent parts of it had acquired a firmer union, so as not to be easily so decomposed by the access of water afterwards."

" A more decisive proof of this decomposition being effected by the acid of the nitrous air being seized upon, is, that the water over which the decomposition is made, does not acquire the least acidity, not even discoverable by the juice of turnsole. In this case, also, there was an *earthy precipitate*, exactly as in the process with the iron filings and brimstone."

' times with a natural flame; sometimes with  
' an enlarged flame; and sometimes with a  
' vehement and crackling noise like dephlogis-  
' ticated respirable air, as if it contained inflam-  
' mable matter; and sometimes perfectly in-  
' flammable, so as to explode like genuine in-  
' flammable air. If the acid was absorbed be-  
' fore the mixture emitted its phlogiston, then its  
' phlogiston would form, with the residuum of  
' the nitrous air, the common inflammable air.

' That this peculiar nitrous air contains  
' more phlogiston than the common nitrous  
' air, is proved from a determined quantity of  
' it phlogisticating a certain quantity of the  
' nitrous acid, more than an equal proportion  
' of the nitrous air would have phlogistcated it.

' The Doctor in his fourth volume, p. 455,  
' mentions a striking fact of the nitrous air  
' turning inflammable, which is a follows:

' I have mentioned a case, vol. I. p. 217, in  
' which nitrous air, after having been exposed to  
' iron, became, not only partially inflammable,  
' admitting a candle to burn in it with an en-  
' larged flame, but was even fired with an ex-  
' plosion, like inflammable air from metals by  
' oil of vitriol. I have since met with a more  
' remarkable fact of the kind.

' At the latter end of September 1778, I had  
' put a pot of iron filings and brimstone into  
' a jar of nitrous air, which, in the course of  
' several days, was diminished by it in the  
' usual proportion. From that time till the  
' beginng of December it had continued with-

' out any change that I had perceived; but  
' about that time, imagining it was increased  
' in bulk, I took exact notice of the dimen-  
' sions of it, and presently found that the quan-  
' tity was certainly increasing. Upon the  
' whole, I concluded that it had increased  
' about one sixth of its bulk, from the state of  
' its greatest diminution. On the 11th of De-  
' cember I examined it, and found it to be  
' proper inflammable air, being fired with many  
' explosions when tried in the usual manner,  
' but they were not so vigorous as those with  
' fresh made inflammable air from iron and  
' oil of vitriol.

' After this, on the 12th of December, I put  
' a pot of iron filings and brimstone to another  
' quantity of nitrous air, and on the 4th of  
' February following it had increased in bulk  
' about one third, and then burned with ex-  
' plosions like the former.'

' The Doctor's opinion of these experiments,  
' as we have observed, was, at first as every one  
' must think, that this nitrous air was phlogis-  
' ticated. He says, vol. III. p. 24.' ' If that  
' which constituted the enlarged flame (speak-  
' ing of this air) be phlogiston contained in the  
' air itself, (and indeed it can hardly admit of  
' any other supposition) it must, &c.' ' In  
' other parts of his works he directly gives the  
' same opinion. But what biassed him to the  
' contrary opinion was, its perfect resemblance  
' to what he calls dephlogistic respirable air;  
' therefore the theory that he had formed con-  
cerning

'cerning this air, made him adopt this singular opinion in opposition to these plain, obvious, and unanswerable facts. For this dephlogisticated nitrous air, can be formed into inflammable air, as he himself found. He says, vol. I. p. 57.' 'By a very easy process I can always make inflammable air from the nitrous acid, viz. by putting iron or liver of sulphur into nitrous air.' 'In most cases the nitrous air in these processes only changes to that state in which a candle burns with a bright crackling flame; but if longer continued it will sometimes become inflammable.'

'As this is an indisputable fact; then if we suppose all kinds of air which will make a candle burn are dephlogisticated, we must be under the necessity likewise of supposing, inflammable air to be a dephlogisticated air, as they are positively one and the same air, only the one being a richer air of the kind from a longer continuance of the process.'

'It will appear singular, that this obvious fact of this nitrous air being phlogisticated, should have been disputed, and likewise that I should have taken so much pains to prove what is so clear. But it is to decide this important question, how these airs support flame? For the established opinion that air supports flame from imbibing phlogiston, made Dr. Priestley suppose that this air was dephlogisticated. But we shall in the course of this work shew, that that opinion is equally as ill founded.'

'Dr.

‘ Dr. Priestley being so imprest with the opinion of this air being dephlogisticated, thinks, ‘ agreeable to his general theory, that its being ‘ capable of supporting flame with so much ‘ vigour is owing to a nitrous vapour, which ‘ must have come from the acid being dephlo- ‘ gisticated. But to wave the convincing ar- ‘ guments, which we have just given to shew ‘ the contrary, let us see how far this idea may ‘ be supported.

‘ If the nitrous air is dephlogisticated, should ‘ we not have expected to have found it acidu- ‘ lated? But wonderful! that it should shew ‘ an acidity before, but now it is not to be de- ‘ tected by the nicest test. The Doctor says, ‘ vol. III. p. 143.’ ‘ The investigation of the ‘ nature of this vapour, (which he supposes ‘ unites with the phlogiston, and from that ad- ‘ mits a candle to burn in it) which though ‘ derived from the nitrous, is not of itself acid.’ ‘ If there is a fact in chemistry, this is one, ‘ that phlogiston neutralizes acids; therefore, ‘ if I take phlogiston, either from the nitrous ‘ acid in its condensed state, or in its state of ‘ vapour, it will make its acidity stronger. To ‘ suppose we should find the acid neutralized ‘ after taking phlogiston from it, is a singular ‘ opinion indeed; but (in our theory) to sup- ‘ pose that phlogiston can perfectly neutralize ‘ an acid vapour, which has received a great ‘ quantity of phlogiston before, is agreeable to ‘ the first rudiments of chemistry, which have ‘ never been questioned.

‘ These

These arguments will hold equally in all points, with the empyreal air made from the nitrous acid and earth. It likewise shews no indication of acidity, either to the delicate test of the vegetable juices, or the alkaline air uniting with it. And just from the very same opinion, and the very self same arguments, they suppose it the nitrous acid dephlogisticated, and formed into an air. But we shall find, that in catching *supposed novel facts*, they have lost sight of the first rudiments of chemistry? for we shall, I hope, make it clearly appear that that *supposed fact* of the respirable air's being phlogisticated when injured, is equally as fallacious as their other opinion, in supposing this nitrous air dephlogisticated. It certainly must appear a very singular hypothesis, when it makes them suppose that the pure nitrous vapour, formed from the dephlogisticated nitrous acid, should be so highly phlogisticated, injuring the common air, (Dr. Priestley shews that it injures it as much as nitrous air \*) and that a vapour formed of one part of nitrous acid,

\* The Doctor, vol. III. p. 193, says, "Lastly, I found, in the course of these experiments, that the power of this red vapour, to phlogistigate common air, was much greater, and acted much quicker, than I had imagined when I made the first observation of the kind. For, after the former observations, I filled another phial with the red vapour, and immediately afterwards opened it under water; when the water, rushing in, filled about half of it, and the remaining air was found completely phlogisticated, not being in the least affected by nitrous air." The vapour, here named, is the pure condensable nitrous vapour.

' acid, and two parts of oil of turpentine, or  
' spirit of wine, would be perfectly dephlogis-  
' ticated like empyreal air; nay, that it should  
' become inflammable and explode, and yet be  
' dephlogisticated. Is it possible to reconcile  
' such contradictions? I should think, the  
' theory that leads them to these opinions, must  
' certainly be erroneous.

' I shall conclude this chapter with the fol-  
' lowing striking facts and observations:

' We cannot help still remarking the very  
' singular deductions and calculations which  
' are made, and may be made, from this theory  
' of respirable air, when injured air is phlo-  
' gisticated. Dr. Priestley supposes that two  
' parts of spirit of wine, and one of the nitrous  
' acid, formed into an air, is a dephlogisticated  
' air. Now, two drops of æther will make a  
' quart of common air explode, and the resi-  
' duum will be injured \*. When, agreeable  
' to Dr. Priestley, this quart of injured air must  
' have received phlogiston in quantity equal to  
' what a pint of inflammable air contains †;  
' and all this must have come from the two  
' drops

\* Dr. Ingénousz says, "I have, perhaps, fulfilled these conditions as near as possible; for all the inflammable air necessary for a pistol, such as Mr. Volta contrived, is contained in the space of one single drop of a liquid. So that a pint bottle may contain as much inflammable air, existing as it were, in a concentrated state, (meaning æther) as is required to fire an air pistol many thousand times."

† Dr. Priestley says, that inflammable air and nitrous air contained equal quantities of phlogiston, from their injuring atmospherical air equally alike, which is in the proportion of one measure of these kinds of air, to two of atmospherical air.

' drops of æther. But yet, which is more extraordinary than all this, the nitrous æther and vapour, (formed of the very same materials and proportions, only the nitrous acid instead of the vitriolic †) they say is dephlogisticated, nay so highly so as to make the most perfect dephlogisticated mixture, equal to what they call dephlogisticated air. If the æther is mixed with empyreal air, these calculations will appear more extraordinary.

' If I fire gunpowder in fixed air, it will explode; if I fire it in atmospherical air, the fire and explosion will be stronger. If in the nitrous phlogisticated, or the common empyreal air, it will still explode more vigorously, and a far greater quantity of fire and light will come from it. Now, is it not rational to suppose that all its fire came from it when fired in the fixed air, and the extraordinary fire and light that came from it when fired in the phlogisticated nitrous and empyreal air came from these airs; their fire and light being decompounded. That it is so, we are certain from both phlogisticated nitrous and

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† Dr. Ingendous says, Priestley, vol. IV. p. 478. "As I make no doubt but this air (meaning the inflammable air made by æther being dropt into common air) is the same that might be extracted from oil of vitriol and spirit of wine by heat, I will give you the following account of the specific gravity of different inflammable airs compared with common air, with which account I was favoured by Mr. Ence: A vessel containing common air to the weight of 138 grains, will contain of inflammable air extracted from iron, 25 grains, of air extracted from marshes, 92 grains; and of that extracted from oil of vitriol and spirit of wine, 150 grains."

the empyreal air before not shewing the least sign of acidity, either with the juice of turnsole or alkaline air, will now after the explosion neutralize the latter, and turn the former red; therefore their acids, which were undoubtedly neutralized by the light and fire after it's discharge, will be free and predominant. The phlogiston which undoubtedly neutralized the nitrous acid, will have been discharged as fire and light, and therefore the explosion was so much louder.

Dr. Priestley's two experiments in p. 68, and 69, likewise seem to shew that the superfluous acid of the nitrous air is attracted by the liver of sulphur, &c. and the phlogiston is left, and for that reason the candle will burn in it, and will sometimes explode.'

The arguments concerning the nature of hepatic air, when mixed with nitrous air, cannot, I think, be answered. Mr. Kirwan has given an excellent paper upon hepatic air, in which he clearly shews that it is sulphur in an aerial state; and that if nitrous and hepatic airs are mixed together, a considerable decomposition will take place. See Philos. Trans. vol. lxxvi. p. 134. Let us accurately attend to this experiment. The nitrous air is supposed to be formed of phlogisticated air, and of a little dephlogisticated air, or the oxygen principle. Now, these two airs receive a great decomposition in the process; a quantity of sulphur being deposited, and the residuum, admits a candle to burn in it with a very

vivid

vivid flame. The sulphur, conformable to the doctrines of modern chemical philosophers, must have attracted the phlogisticated air of the nitrous air, leaving the oxygen principle: But sulphur has no attraction for phlogisticated air; for, in burning sulphur in atmospherical air, the dephlogisticated, and not the phlogisticated air is attracted: The sulphur and dephlogisticated air being supposed to form the vitriolic acid.

To suppose, that nitrous air consists of dephlogisticated and phlogisticated airs, must appear very extraordinary, when we reflect, that these two airs are so abundantly generated in nature, by vegetables and animals, and by art; so that there is scarce a process which produces the one, but also produces the other: viz. the air from nitre, &c. That nitrous air is not formed of these two airs, is clear, from the experiment of passing this air through hot earthen tubes. The nitrous air in this process will be all turned to phlogisticated air.

Every old chemist, who has the least knowledge of the affinities of bodies, must be struck with surprise to suppose, that such highly phlogistic bodies as sulphur, the caustic alkali aerilized, and nitrous air, should form so large a residuum of dephlogisticated air. Mr. Kirwan's experiments convince him, that it is the acid which produces the precipitation of the hepatic air, and therefore, he neutralizes it with alkaline air. But Mr. Kirwan will find another fact, which is, that the more the

alkaline air is employed in this process, the more easily will the air admit a candle to burn in it with greater vividness: But it should have been the reverse. In a word, the ideas, which most of our modern chemists entertain of phlogistication and acidifying airs, are the very reverse to what they have supposed; because, in direct opposition to this old chemical fact, that alkalies and phlogiston neutralize acids. And this other important fact may be added; namely, that if this dephlogisticated nitrous air is imbibed by the pure nitrous acid, it will highly phlogisticate the acid; whereas it should have dephlogisticated it.

I should think enough has been said to convince every chemist unprejudiced by the theories of modern chemistry; though alas! they appear not disposed to see the truth.

Having formed nitrous air from the acid and vegetable oils, I added a quantity of alkaline air to it. Two motives induced me to do this. To neutralize and decompose that part of the acid, which was but loosely attracted; and likewise, to neutralize that part of the acid, which would still adhere to the nitrous air. And I proved, by experiments, that part of the alkaline air was precipitated as nitrous ammonia, and likewise, that part still adhered to the nitrous air; and, upon examining the air, I found it was what is called the dephlogisticated air, admitting a candle to burn in it. But, need chemists be told, that it is nitrous air more highly phlogisticated? Nay, in this

this same process, it is sometimes so highly phlogisticated, as to explode like the pure inflammable air; burning with such vivid flashings, (as Dr. Harrington justly observes) as to resemble partial explosions. I have found in the nitrous air from vegetable oils, a quantity of fixed air; and this fixed air, upon alkaline air being added to it, is not precipitated, but united to the alkaline air: And they likewise seem to add to the quantity of the air, which allows a candle naturally to burn in it.

After having received so much phlogiston or alkaline air, as is sufficient to deprive it of the power it had when nitrous air, of turning litmus red, it then becomes oxygen gas; and this oxygen gas is supposed to reside in the acid, so as to produce its acidity; for as it loses that, it becomes more like nitrous air, or the acid less conspicuous; but it ought to have been the reverse; and it is evident that, when it has this quality of admitting a candle to burn in it, it becomes less acid; the acid being more saturated or neutralized with phlogiston, or alkali, which is the same as phlogiston; but only (as Dr. Harrington has proved) less concentrated.

Can any one possibly be mistaken of this nitrous dephlogisticated air, even from Mr. Kirwan's history of it? Indeed, after reading Dr. Harrington's account, it was impossible for Mr. Kirwan, or any other chemist, who was in the least acquainted with chemical principles, to suppose it was dephlogisticated: But that

that disposition, which has been the ruling mark of our *aerial philosophers*, is, to make it a point not to name Dr. Harrington. What does Mr. Kirwan do? he does not make or call this air dephlogisticated, but calls it deacidified. We have got a number of new terms into chemistry from their extraordinary ideas of it. But I suppose he means by this, the air is more neutralized; could he not have said, agreeable to Dr. Harrington, more phlogisticated. But even to take his own term, deacidified; what bodies were there to deacidify it but the sulphur and alkaline air? And as, according to their hypothesis, air that will admit of the life of combustion, (call it dephlogisticated, deacidified, or what they will) it is, they say, the acescent principle or principles of acidity. Then, must not it appear to form a chasm in reasoning, to suppose that an air which has got its acid taken from it, should, from that cause, become the acescent principle? But such are their absurdities.

But to proceed with the arguments. Nitrous air exposed to phosphoric hepatic air, and treated in the same manner as the common hepatic air, will likewise become the dephlogisticated or deacidified air of modern chemists. But then, if this phosphoric hepatic air is burned with atmospherical air, which it will do without the aid of any other combustible matter; the atmospherical air will be left in the very same situation, as if any other combustible body had been burned in it; or,

if

if the phlogiston of phosphorus or alkali had been burned in it. Those who oppose this true and not obscure doctrine, cannot require more convincing proofs; but hitherto they have shut their eyes: This is an important fact. There is an air, called deacidified or dephlogisticated, (for modern theorists have given it both these names) that acts upon atmospherical air, in the same manner as any combustible body. Now, this is a good proof in favour of Dr. Harrington's theory, that atmospherical air and combustible bodies, are both phlogistic bodies, since the phosphoric hepatic air acts in one process, in the same manner as dephlogisticated air, in admitting, the same as atmospherical air, a candle to burn in it: And in the other process, it acts upon atmospherical air, just as a candle would do by burning in it, and making it foul air, and that to a considerable degree; a small quantity of phosphoric air, acting upon a large quantity of atmospherical air \*.

One of the most ancient, and best established facts in chemistry is, that acids and alkalies attract each other. Our modern chemists have done all they could to destroy this fact; nay, by introducing new principles, they have hewn it down, with all the rest, as a cumberer of the ground.

\* The phosphoric and dephlogisticated airs acting, agreeable to our hypothesis, in one case as the agent, in another as the principle; therefore, certainly we must suppose, that there is not that difference in the composition of the agent and principle.

ground. Modern experiments and theories say, that alkalis do not attract acids. But heat, which is their great agent in finding out the affinities and compositions of bodies, they say, proves it: For, in exposing nitre to heat, the alkali attracts the phlogisticated air, and the oxygen principle is aerilized. But this is more particularly singular in the nitrous ammoniac, as the volatile alkali is so volatile. Yet, they say, by exposing the nitrous ammoniac to heat, the decomposition of the acid takes place, its phlogisticated part being attracted by the volatile alkali; and this not only occasions the loss of the alkali's attraction for acids, but even the loss of its volatility. But to advert to experiments, which, in the opinion of aerial chemists, can explain every thing.

Upon exposing the nitrous ammoniac to heat, a very great abundance of this dephlogisticated nitrous air is generated. But if chemists would wish only to see truth, let them take this same air produced from the nitrous ammoniac, and expose it to the dephlogisticated nitrous acid, and then they will know, whether their theory, or that of Dr. Harrington, is just. If the air is dephlogisticated, upon the acid's imbibing it, the acid will then become more acid or dephlogisticated; but if Dr. Harrington's theory is just, it will become the same, as if so much of the volatile alkali, and a little nitrous acid, (which he says the air contains) were added to it. Upon exposing them together, the acid imbibes the air, so as to produce the

the very same phenomenon, as if I had added a quantity of volatile alkali to it. Or, if this same dephlogisticated air is exposed to the calx of iron, the calx will attract it; and upon examining the calx, it will be found to have imbibed the pure nitrous ammoniac. In burning the calx, it will injure or phlogisticate, as they call it, atmospherical air, just the same as the nitrous ammoniac.

Can aerial chemists, after this review pass by Dr. Harrington's theory, as not deserving notice? If they do, it is evident, they are not willing (however much convinced in their own minds) to acknowledge to the world, that they have been mistaken. But chemical philosophers who will not attend to truth, when it is told them, do not deserve the name.

This singular theory of theirs destroys every old chemical fact. The vegetable juices turn red by being exposed to an acid. This is a test by which to know, whether or not, nitrous air is more acidified, when it is dephlogisticated. Litmus, by being exposed to nitrous air, turns red; but if this nitrous air is formed into dephlogisticated air, it will not turn the litmus red, but rather green. Can this extraordinary doctrine be supported by any fact? For, when this dephlogisticated air, as they call it, is exposed to the strong concentrated nitrous acid, the stronger acid will attract the alkali from the weaker; because the acid, by being phlogisticated, loses its strength. This

is the case, when the nitrous acid is phlogisticated in a condensed state; for, by adding a stronger, or dephlogisticated nitrous acid to a body which has attracted it, it will expel it.

Every chemical process is a proof to confirm this truth, that fire is chemically united to the different acids; particularly when acids are mixed with earths or alkaline salts. When the vitriolic acid, metallic, or other earths, are exposed to a strong heat, they become aerilized into a high phlogistic air, improperly called dephlogisticated: But will also yield under the same process, phlogisticated and fixed air. The vitriolic acid, conformable to the present theories, is only a compound of dephlogisticated air and sulphur, and therefore should yield neither fixed nor phlogisticated air; and surely, they will not allow the earth of alum to be a compound of either of these airs. The nitrous acid will also, with the different earths, yield these airs; and the marine acid is so strongly attracted by these earths, that no heat can comparatively volatilize it. A strong heat is required to volatilize the phosphoric acid; and therefore, the air which it produces, will be inflammable air. See Dr. Priestley. With the calx of lead and this acid, he produced nothing but inflammable air, or air of a higher impregnation: But obtained it in great abundance.

The phosphoric acid, conformable to Mr. Lavoisier, consists of 69 parts of dephlogisticated air, and only of 31 of its peculiar basis; and,

and, conformable to Mr. Kirwan, of 69 of fixed air, and of 31 of its basis. Now, if this acid is distilled with the calx of lead, minium for instance, inflammable air only is produced; all the dephlogisticated air, which the phosphoric acid and the minium are supposed to possess, has disappeared. What is become of it? It cannot be in the lead; for, if the fire is pushed, the lead is reduced. Is not this hostile to their hypothesis, and friendly to that of Dr. Harrington? That gentleman has called upon them to explain these phenomena; but they have not, nor can they explain them.

The vegetable acid, which is so much weaker than the preceding ones, forms with earths, only phlogisticated air, or air with a less impregnation of fire. The aerial acid united to earths, as in lime-stone, magnesia, &c, forms, by means of a strong heat, different kinds of air; viz. fixed air, phlogisticated air, dephlogisticated air, and inflammable airs. But according to their hypothesis, it should form only dephlogisticated air, phlogiston or carbone. The electric spark taken in fixed air, will produce all these different airs, the same as lime-stone or magnesia. In all these productions, the airs generated, shew very forcibly, that this is owing to the concentration of fire. For the airs are produced in that regular order, as is conformable to the quantity of fire they possess. As a proper example of this, let us take the air from lime-stone. First, fixed air is

expelled without any impregnation; then phlogisticated air, with sufficient to neutralize the acid; then dephlogisticated air, with a still higher impregnation; and at last inflammable air, with the highest concentration. But fixed air, when united to the calx of mercury (which, conformable to Dr. Harrington's hypothesis, is more easily aerilized, and therefore more adapted with acids, to form air with a higher concentration of fire) will form only dephlogisticated air.

There is a process, by which fixed air may be all formed into a phlogisticated air: This is done by impregnating water with it, and then expelling it again by heat. There will always be a residuum of this phlogisticated; and by repeating the process upon the same air, you at length bring it all to this kind of phlogisticated air. As there is nothing but water and heat used in the process, we cannot possibly mistake it. It could have received only these two bodies: Moreover, the quantity exactly corresponds with receiving only a small addition, conformable to the alteration which these two bodies would make upon it. Dr. Priestley likewise, by exposing it to phlogistic processes, turned it to this air.

But, a very decisive experiment, an experiment impossible to be mistaken, is, the passing fixed air through heated manganese. By repeatedly passing, it becomes at last dephlogisticated air. That phlogisticated air is a medium, between fixed and dephlogisticated air, is

is thus proved: It first becomes phlogisticated, before it forms what they call dephlogisticated air, receiving each time in passing more concentrated fire, till it at last becomes highly dephlogisticated: This we have elsewhere more fully explained.

Now, I ask those who have the least knowledge of chemistry, Can they possibly mistake facts so obviously plain? It is impossible, if we retain true notions of what is just.

If I add oils, volatile alkali aerilized, or phlogiston in any other form, to the mineral acids, the nitrous, vitriolic, and marine airs will be formed: But these airs, say they, are the acids, having part of their oxygenous principle taken from them. But, as I before observed, if these airs are absorbed by the same acids, from which they were generated, they will clearly appear to be the phlogisticated acids. But, as phlogiston is, with many chemists, a vague term, and not well understood by them; if I add the aerilized volatile alkali, it being highly phlogistic, to the mineral acids, they will produce the same phenomena; namely, nitrous air, and phlogisticated acids.

Then, in this case, are we to lose sight of the first rudiments of chemistry, and of one of its chief principles; that acids and alkalies have a chemical attraction for, and neutralize each other? Were our chemical ancestors mistaken? Does the volatile alkali not unite to the acid, but decompose it, taking from it's oxygenous principle, which, with the alkali, forms fixed

fixed air, according to Mr. Kirwan? Nay, moreover, our ancestors in chemistry, according to them, were still farther mistaken; for they supposed that the stronger acids would expel the weaker: But here the fixed air is retained by the compound, together with the nitrous acid, so that the nitrous, volatile, marine airs are expelled; the fixed air remaining snug with the mineral acids. But a still greater difficulty remains: For, if the volatile oils, or alkali, by being united to dephlogisticated air, do form different bodies with it, agreeable as the alkali is made inflammable; so it produces, when burned with the dephlogisticated air in one state, fixed air; but if in the state of inflammable air, (which it will form, either by heat or the electric spark) the nitrous acid will be formed. Here then, (as Dr. Harrington very properly observes) the nitrous acid, by losing its phlogisticated air, and receiving the volatile alkali in its stead, is again formed. Can we reconcile such absurdities?

But some say, that the nitrous acid produced in this process, is from the union of the dephlogisticated and phlogisticated airs. However, Dr. Priestley has shewn in all his chemical writings, that dephlogisticated airs may be turned into phlogisticated airs, by what he calls the phlogisticating process. See Priestley.

Then, exactly conformable to these experiments, and to their hypothesis, dephlogisticated air and phlogiston form the nitrous acid. To shew this in a conspicuous light, I take twenty

twenty measures of dephlogisticated air; half of this I phlogistigate by phlogistic processes, and then unite it to the other half of the dephlogisticated air; and after that, by taking the electric spark in them, I must form them into the nitrous acid. Or, to reverse the experiment, take twenty measures of phlogisticated air, and form the half of it into dephlogisticated air, by exposing it in water to the rays of the sun; then add the remaining half of the phlogisticated air to it, and we shall be able, with the electric process, to form them into the nitrous acid. Hence, we are under the necessity of giving our assent to this truth, which Dr. Harrington has always taught, viz. that both phlogisticated and dephlogisticated air contain an acid. It must therefore appear very extraordinary to suppose, that dephlogisticated nitrous air, is not the nitrous acid aerilized with phlogiston: For, take this air, and carry it through the self same processes above mentioned, and the proof will be found to be equally strong and fully shews itself to be formed of the nitrous acid.

Mr. Kirwan will soon be convinced of the justness of these arguments; for he himself expressly says, that the nitrous dephlogisticated air is *not dephlogisticated*, but deacidified. After what Dr. Harrington has said, he must be convinced of the error of calling it dephlogisticated.

Mr. Lavoisier's opinion, that inflammable and dephlogisticated airs form water, must, when candidly considered, be thought very extraor-

extraordinary. He excludes phlogiston, and invents another imaginary principle in it's stead, calling it *carbone*; and supposes that this *carbone*, with dephlogisticated air, form fixed air: Therefore, this *carbone* must be very general, as most processes in nature form dephlogisticated air into fixed air: The burning of almost all combustible bodies; the putrefaction and respiration of all bodies; and likewise vegetation. So then, no body upon this globe can undergo any change, without imparting this supposed coal to the air. The present received chemical doctrines have given rise to, and sanctioned such extravagant opinions. When a science is in it's infancy, it is surprising to think what vague and ill-founded opinions men form of that science! And, as a proper attention to the history of any discovery in science, evinces the truth of this observation, so it is particularly true of the present doctrines respecting air. This is an enlightened age. The prejudices and imperfections of dark periods are now removed. Every person of learning and science professes, that all his researches are to find out truth, and to receive it by whomsoever offered. How unpardonable then must it be in such persons, not to have received the truth, when it has been so clearly pointed out to them.

Chemists say, that by burning bodies in dephlogisticated air, the air is turned to water, to phlogisticated and fixed airs. In the respiration of animals, in the putrefaction and combustion

combustion of bodies, the air is necessary; because, it either imparts, or receives something from the bodies acted upon by these processes. But the present chemists, wandering from one error to another, have set aside this simple doctrine, and have given us a very singular one; viz. that the air in some processes of combustion, receives a body, which they call inflammable air; and in others, a body called *carbone*. In this manner, to be sure, they make these bodies the two great elements in nature. But we find the air in some processes of combustion, respiration, and putrefaction, turned to what they call phlogistinated air. In Mr. Lavoisier's system, there is no theory to account for this fact: However, the supporters of phlogiston say, all these changes of the air in these different processes are made, by the air's receiving one and the same body, called phlogiston; or, which I should rather call the phlogistic system: Therefore, conformable to the phlogistians, dephlogistinated air and phlogiston do, by their chemical union, form water, the nitrous acid, fixed and phlogistinated airs. Can we, as chemists, possibly reconcile such a paradox to truth? A paradox, which supposes that water, the nitrous acid, fixed and phlogistinated airs, are one and the same body, formed of the same ingredients: Such, however, are their opinions.

Upon a full view of the present chemical theories, we are forcibly struck with the extraordinary doctrines contained in them: Doc-

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trines, to which chemists could certainly never have been reconciled, had they not wandered from error to error. Were their principles to be unfolded all at once to a pupil of the old school, he would think them the most extravagant that could be adopted. But most of the great names, which rank so high in the present chemical world, belong to men, who were not regularly bred in the old school; for if they had, some of its fundamental and most evident principles, would not have been set aside for aerial flights.

There are a great variety, or different kinds of inflammable air. This air is procured from metals, charcoal, and spirits of wine; essential oils, and olive oil, by their vapour passing through earthen tubes. All these seem to be high phlogistic bodies; as they will reduce the calces of metals, form the nitrous acid into nitrous air; and likewise inflame with dephlogisticated air. These inflammable airs seem to be one and the same, as having the same general characteristics. There are many others, though formed by different processes; viz. inflammable alkaline air, and the marine acid air, formed into inflammable air: See Priestley. But these likewise have all the characteristics of inflammable air.

These different inflammable airs, leave very different residuum, when burnt in dephlogisticated airs: And therefore some modern chemists, according to the theory received by them, will have these to be different bodies; the

the one kind formed of inflammable air, and the other having no relation to it, but formed of an imaginary element, called coal or *carbone*. On the other hand, several modern chemists believe there is such a body as phlogiston, and that of this body all inflammable airs are principally formed.

But to adopt their principles, or to suppose that the nitrous acid, water, fixed and phlogisticated airs, are all the same body, would be to adopt greater singularities, and to encounter greater and more numerous difficulties, than we could be taxed with, or exposed to, by adhering to the doctrines of the old school. Let any chemist try to reconcile such contradictions as we must assent to, by maintaining that these different airs are all formed of the same body. I have very clearly proved by a variety of experiments, and likewise from their reducing metals, that the heavy inflammable airs, by having a greater quantity of fire thrown into them, will become considerably lighter, and act upon dephlogisticated air, the same as the light inflammable air upon metals. This is done by passing the electric spark through them; or by exposing them to intense heat; or by putting red hot bodies into them: Or, those inflammable airs which are produced from vapour, by passing the vapour through a larger surface of earthen tubes. By treating the air produced from the same body in these different ways, when fired in dephlogisticated air, I have found that they

would form different residuum, viz. the nitrous acid, water, fixed and phlogisticated airs.

But not to follow the practice of others, by dwelling upon the history of dry experiments, I will give you the result of my own: And here let me obviate an objection that may be made. It perhaps may be objected, that my knowledge of experiments is not so complete, as to take them upon my own opinion. I answer, Let the experiments be repeated again and again, and I make no doubt, but that the results will be found to be such as I have stated them. Chemists have paid too much attention to experiments, whilst they have neglected the knowledge proper to interpret them; and the consequence of this has been, that those who rank high in the chemical world, have adopted the most absurd hypotheses. I do not mean by this observation, to detract from the merit of many great men, whose chemical experiments do them immortal honour; no, I only want them to listen to the voice of reason, of truth, and of justice: For, if they shew no more candour than they have hitherto done, their accounts with science will be, I am afraid, more upon the debtor than the creditor side. Dr. Harrington has been treated in a very ungentle (I had almost said cruel) manner. Those who differ from him in sentiment have, instead of a candid examination of his experiments of investigation, done all they could to damp and restrain them.

Inflammable

Inflammable airs burn in dephlogisticated airs with an intenseness proportioned to their purity, or to the quantity of their concentrated fire; and they will accordingly decompose dephlogisticated air. If they possess a great quantity of fire, as the inflammable air from metals, they will entirely decompose it, and form the nitrous acid and water; but if they do not possess so much fire, part of the dephlogisticated air will be left in the state of phlogisticated and fixed airs. I have found in some experiments a great quantity of fixed air formed; but by throwing into those airs, which are formed by passing through hot earthen tubes, a greater quantity of fire, either by heat, or the electric spark, and by enlarging the surface of the tube through which the vapour had to pass; all the inflammable airs, by having more concentrated heat thrown into them, will act accordingly upon the dephlogisticated air. And also, by a proper adjustment of the proportion of the inflammable airs to the dephlogisticated airs, I could accurately ascertain *a priori*, what would be the residuum; and clearly deduce this conclusion, that inflammable airs are concentrated fire; and that they (conformable to Dr. Harrington's hypothesis) will act upon the dephlogisticated airs, in proportion to the quantity of fire set loose. In the pure inflammable airs from metals, the fire is separated all at once, so as to produce an explosion; but in the heavier airs they burn more gradually: However, if a greater quantity

tity of fire is thrown into them, they will likewise explode.

The volatile alkali burnt with dephlogisticated air, forms principally fixed air. From Mr. Milner's experiment of passing the volatile alkali through a red hot barrel, in which was manganese, it is evident, that as the heat expelled the dephlogisticated air, the alkali was set on fire by it; and being consumed in that hot tube under so great and intense a degree of heat, the dephlogisticated air will be decomposed into the nitrous acid and water, which, acting upon the iron barrel, form nitrous air. Heat, by expanding the heavy airs and making them lighter, acts here as one might expect. There is no occasion to have recourse to Dr. Austin's laboured explanation of heat separating the two airs, in which he supposes the heavy inflammable airs to be composed of inflammable and phlogisticated airs.

That metals contain phlogiston, is proved from this very simple experiment. By exposing iron to water and dephlogisticated air, the iron will be turned into a calx, and a phlogistic oily body will swim upon the top of the water. As our modern chemical theorists are always ready to adjust every phenomenon to their theory, how absurd soever it may be; let us see if this experiment is favourable to them. I grant that this oil is inflammable air condensed; but, conformable to their theory, they must also allow, that the water was decomposed in this process; the oxygen principle entering into

into the calx, and the phlogiston or inflammable air being separated. But here we are aground; for the dephlogisticated air which certainly forms the calx, is imbibed by the calx, and so disappears. For, if there is no dephlogisticated air in the process, the iron will not be formed into a calx. This same oily phlogistic body will reduce the iron again; or, if exposed to a great heat, will form inflammable air. And more particularly, if this calx should be exposed to the parisian minor, it would be reduced without addition, and would undergo the same process again with dephlogisticated air and water.

That it is the dephlogisticated air that forms the iron into a calx, is probable from all the doctrines of chemistry. It cannot be supposed it is from the water being decompounded, as the dephlogisticated air was free and disengaged, while the water's supposed dephlogisticated air was united to inflammable air: But if even they should contend for it; the dephlogisticated air of the atmosphere should have united with the inflammable air of the water, and formed with it water again.

Mr. Kirwan, in endeavouring to reconcile the phlogistic theory with Mr. Lavoisier's, upon the formation of acids, has fallen into great errors. He supposes that inflammable and dephlogisticated airs form water; that metals are formed of phlogiston and the earth of the calx; and that the calx is formed of fixed air and the earth of metals. Now, this doctrine,

doctrine, though Mr. Kirwan is not aware of it, directly contradicts Lavoisier's. The former supposes that fixed air is formed of the same kind of inflammable and dephlogisticated airs, which form water: For the inflammable air obtained from metals, and which will reduce them again, when fired with dephlogisticated air, will form water. And moreover, when metals are reduced without addition, their being decompounded, is, he says, from the fixed air contained in them; the dephlogisticated air being set loose from it. But can Mr. Kirwan possibly, as a chemist, reconcile the absurd idea, that fixed air and water are formed of the identical selfsame bodies, and by the identical selfsame process, viz. combustion: But this I do positively declare, that our aerial chemists can reconcile any thing, if ever so preposterous. They are so much attached to their experiments in gun barrels, retorts, &c. imagining some charm in them, (which are to discover every thing) that I can compare their credulity as being equal to that which appeared in the affair of the bottle conjurer in the Hay-Market. Dr. Harrington says in his Letter, p. 4. "Dr. Priestley, in the great variety of his experiments, proves, that in some cases the burning of these two airs produces water, and sometimes fixed air.

" Then let us just compare fixed air with water. Fixed air, when condensed, neutralizes alkalies and calcareous earths; will any chemist suppose water to do the same? When fixed

fixed air condensed in the vegetable alkali, is just upon the same footing, in respect to solidity and being aerilized, as the water is in the same vegetable alkali. And if our fathers in chemistry should be told, that the water and the fixed air in the salt were both the same chemical bodies, composed of condensed dephlogisticated, and inflammable airs; would they believe it?"

I shall now take a view of those chemical writings, with which Dr. Priestley has favoured the world since the publication of Dr. Harrington's Letter: But the reader will allow me to make a previous observation; which is, that Dr. Priestley has been very careful, not to mention that gentleman as a fellow-labourer. What reason shall we assign for his silence? The question, I think, may be very easily answered. There is an opposition of hypotheses; and, if Dr. Harrington's is the true one, Dr. Priestley's must of consequence be false: However, not to mention the chemical doctrines of his antagonist, is, in my opinion, very wrong: Fair discussion is the best way to know who has truth on his side; let then the two hypotheses be candidly canvassed by those of an impartial public, who are able to judge. Will it be said in extenuation, that Dr. Harrington's theory deserves no answer? Were any man, who in the least pretends to the name of a chemist, to make such an assertion, I should not scruple confidently to assert, that he knows nothing of chemistry.

It is very possible, after the usage Dr. Harrington has met with, that my labours may receive the same treatment. However that may be, I publicly call upon modern chemists, (some of whom deserve the highest merit, and their works will be esteemed as long as true science lasts) not to shrink from the present investigation, but come boldly to it. If they do not, their labours, instead of promoting science, will rather retard it. For, when a person, presuming upon the reputation he has obtained in the world, endeavours by an uncandid behaviour to draw the curtain over truth; this conduct will in the end counterbalance all his labours. I throw down the gauntlet, as Dr. Harrington has done; and, if none takes it up, it must be for fear of being foiled; or to drop the metaphor, I openly avow my chemical principles, and challenge a fair discussion of them. But should Dr. Harrington's theory, and these my well meant attempts in favour of it, continue still to be passed by with a contemptuous silence; and should time, which does justice to philosophers and their principles, shew our's to be right; in this case, to avoid discussion, which leads to truth, is worthy of blame, and posterity will, in this instance, undoubtedly condemn their conduct. I might mark such behaviour with its proper stigma: This, however, I will not do, but leave the reader to make his own reflections.

This age is with great propriety called enlightened: It is the age of science, and the ma-

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ny discoveries made in it, have been happily applied to the purposes of human life. Nay more, it is the age of truth, and philosophers, both natural and moral, profess to have only truth in view, in all their investigations. Hence, to me it is matter of surprise, that no old chemist has before me, paid a proper attention to Dr. Harrington's principles! For, if a theory which accounts for, and proves by solid reasoning, all the chemical phenomena, in opposition to principles contrary to nature and reason, and which account for none of the phenomena, should be attended to and adopted, that gentleman's is the one: And if any chemist is not disposed to adopt it, let him at least treat it with the candour it deserves. Dr. Priestley is anxious to know what he breathes, before he ceases to breathe. When this is told him, he will not, I hope, think it below him to accept of information.

Dr. Harrington clearly proved as long ago as the year 1780, that an acid and water are neutralized with fire, and aerilized into atmospheric air. That in respiration, this fixed fire is attracted by the blood from the acid and water; that the acid is left in the state of fixed air, and a great quantity of the water is condensed in the process. That in putrefaction, the air undergoes a similar decomposition, and the fixed fire is attracted by the putrid body, so as to become putrid or alkalescent. That in combustion, the fixed fire (as we have prov-  
ed) is set loose. These discoveries were only a

prelude to those of the first principles of animal and vegetable life, the phenomenon of animal heat, with other secondary phenomena. He published in 1785 a full history of the different airs, clearly shewing the formation of each. That the air is again renewed after being injured, he proves from it's levity; owing to which, it is taken up into the higher regions of the atmosphere, where the fixed air and water are again saturated with fixed fire, by which, becoming more specifically heavy, it descends again. That phlogiston is fixed fire chemically attracted; and that it is capable of being set loose again, by various processes in nature\*.

Now, I scruple not to declare, that all the principal and leading discoveries respecting atmospheric

\* I would beg leave to mention, that Mr. De Luc, after Dr. Harrington, supposes, that the air is renewed and purified again in the clouds; and he endeavours to account for it upon the ridiculous hypothesis of water being formed of inflammable and dephlogisticated airs, which is decompounded in the clouds. But even this supposition was, after Dr. Harrington had proved by experiments, and published to the world, that the clouds are nature's laboratory, as he significantly expresses it. Of this discovery Mr. De Luc takes no notice, but mentions it as his own opinion. And though Dr. Harrington, as far back as 1780, clearly proved, that fire is capable of being chemically attracted; yet Mr. De Luc, long after this, speaks of it as his own observation, and so has the merit of both discoveries. Such is the usage that gentleman has received from those, who, very probably, were indebted to him, for discoveries they call their own. Moreover, Mr. De Luc follows Dr. Harrington closely in the arguments, to prove the homogeneousness of the atmospheric air, though, conformable to the general policy of his conduct, he never once mentions his name.

mospherical air, its formation and composition, with the manner in which it supports animal and vegetable life, and the life of combustion; I say, that all these are fully shewn by Dr. Harrington. The only thing that appeared to me rather obscure was, the life of combustion; but this, I hope I have, conformable to Dr. Harrington's principles, fully demonstrated in this Treatise. I should be sorry to endeavour, like some chemists, to take away any part of his merit. However, I venture to predict, that the time is fast advancing, when every thing relating to this chemistry, will be properly understood, and settled upon a right foundation. Dr. Harrington has in his Letter (printed in 1788) very clearly and fully detected the errors of his opponents, concluding it with this pointed language, but as yet he has received no answer: "I hope I have made it appear in this Letter, and in the rest of my writings, that the present hypotheses of philosophers account for none of the phenomena we have considered; and that mine give an explanation of them all, both consistent and satisfactory."

"Therefore, gentlemen, I thus publicly call upon you, either to vindicate your opinions or renounce them; science and the public claim it of you."

There is an experiment mentioned by Dr. Higgins, which he says is in favour of Mr. Lavoisier's system; as indeed we must agree with him that it is directly contradictory to  
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the system of phlogiston, with the belief that dephlogisticated air is, in reality, phlogisticated. But if Dr. Higgins and others will please to see the truth; this dephlogisticated nitrous air is formed of the nitrous acid and the phlogiston of the copper. It will be a strong proof in favour of the doctrine of phlogiston. The Doctor says, p. 550. "If tin be introduced into a neutral solution of the tin in the nitrous acid, it is calcined, a calx is thrown down, and the dephlogisticated or imperfect nitrous air is produced. Dephlogisticated nitrous air, according to the phlogistians, contains no phlogiston; then I ask, what becomes of the phlogiston of the newly calcined metal? If tin contained phlogiston, either inflammable air or nitrous air would be produced, or a portion of the dissolved tin would be precipitated in its metallic state; neither of which will take place, if the experiment be well conducted. Hence I should suppose, that metals do not precipitate each other in their metallic state, in consequence of a double affinity proceeding from the matter of light inflammable air, (or phlogiston) and likewise that metals part with no such thing during their calcination in acids."

Dr. Priestley, after Dr. Harrington's publications, gave different papers to the Royal Society, in which he endeavours to prove that water is not formed of inflammable and dephlogisticated airs. His arguments and experiments are the very same as those of that ingenious

genious chemist; therefore, we shall say nothing more to refute so absurd an hypothesis, which supposes these airs form the composition of water: Only he has in these papers blended an absurd hypothesis, and much false reasoning, with the truth. That learned chemist has in these papers given, with his luxuriant pen, a multiplicity of experiments and reasons: And, as an experimenter, he has certainly great merit; but as a chemical reasoner, I can by no means pay him that compliment. In reviving the red precipitate in inflammable air, he found a small quantity of fixed air left in the residuum; and from thence he, conformable to his singular hypothesis, supposes that inflammable and dephlogisticated airs, in their nascent state, form fixed air. But the fixed air produced was not equal to what should have been produced from the quantity of the two airs employed, it being ten times less. But this is such a trifle with modern chemists, as not to give the least shock to their hypotheses. Had Dr. Priestley but attended to what Dr. Harrington says, he would have found the reason why fixed air was formed. But to have done so, would have led him to renounce his former opinions. For the reason why fixed air is formed in the process, see p. 75.

March 26th, 1789, Dr. Priestley presented another paper to the Royal Society, in which he attempts to prove that acids contain dephlogisticated air, as one of their constituent parts. He has followed Dr. Harrington in contradic-  
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ting the opinion of the supposed formation of water. But in this paper he sticks to an opinion (equally absurd) that the acids are formed of airs. To have given up this would have confirmed Dr. Harrington's theory; he therefore exerts all his powers of rhetoric in it's favour. We shall now investigate what he advances.

Though Dr. Priestley proves in all his former chemical writings, that heat phlogisticates the nitrous acid, and says, vol. V. in a summary view of his principal facts, "Spirit of nitre may be procured almost colourless by a careful distillation in the common way, iv. 453. From being of a deep orange, it becomes green by long keeping, 453; afterwards of a deep blue, 454. But if it be exposed to the open air, it becomes coloured again, ib.

Heat deepens the colour of this acid, iii. 249. It's colour is universally owing either to phlogiston or heat, iv. 2." Yet, as these facts are hostile to his present chemical opinions, and as he sees the danger they are in from them; he endeavours to account for the acids being formed of airs, in a most *extraordinary manner indeed*. The reader will, I hope, indulge this observation, namely, that our present chemists by being bred up in the aerial school, are ready to receive any hypothesis, however extraordinary it may be \*. He talks of heat, as it were

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\* In my former experiments, vol. IV. p. 2. I found that the colourless acid became smoaking, or orange-coloured, and

the phlogiston previously contained in the acid. Chemistry knows no such ideas as those of evolving, without some chemical attraction or influence. But he afterwards found it to be light only that produced this effect; and again he found it to be heat: So fluctuating are his opinions. He gives in vol. V. some

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and emitted orange-coloured vapours, on being exposed to heat in long glass tubes, hermetically sealed; and I then concluded, that this effect was produced by the action of *heat*, evolving, as it were, the phlogiston previously contained in the acid. Afterwards, having found that it was not *heat*, but *light* only, that was capable of giving colour to spirit of nitre, contained in phials with ground stoppers, in the course of several days; and that in this case the effect was produced by the action of *light* upon the *vapour*, which gradually imparted its colour to the liquor on which it was incumbent. (see vol. V. p. 342.) I was led to suspect, that as the glass tubes, in which I had formerly exposed this acid to the action of heat, were only held near to a fire, in the day-light, or candle-light, it might have been this *light*, which, in these circumstances, had, at least in part contributed to produce the effect.

In order to ascertain whether the light had had any influence in this case, I now put the colourless spirit of nitre into long glass tubes, like those which I had used before, and also sealed them hermetically, as I had done the others; but, instead of exposing them to heat in the open air, from which light could not be excluded, I now shut them up in gun barrels, closed with metal screws, so that it was impossible for any particle of light to have access to them; and I then placed one end of the barrels so near to a fire as was sufficient to make the liquor contained in the tube to boil, which I could easily distinguish by the sound which it yielded. The consequence was, that in a short time the acid became as highly coloured as ever it had been when exposed to heat without the gun barrel. It was evident, therefore, that it had been mere *heat*, and not *light*, which had been the means of giving this colour to the acid, and which has been usually termed *phlogisticating* it. See Priestley, Phil. Trans. vol. lxxix. p. 139.

striking facts which shew, that the heat acts principally upon the vapour of the nitrous acid, phlogisticating it. But in this paper to the Royal Society, he supposes that the heat acts upon the nitrous acid, expelling the vapour, as being dephlogisticated.

Mr. Kirwan attempts to refute that excellent chemist, the immortal Scheele, who endeavoured to prove that the fire of the sun phlogisticates the nitrous vapour. Mr. Scheele says in his Experiments, p. 80. “*d.* I poured a small quantity of the purest fuming spirit of nitre (No. 25) into a white phial made of crystal glass, provided with a glass stopper, and exposed it to the light of the sun. Three hours after I found the phial filled with red vapours; the same happens when the phial is set on a German earthen-ware stove; but it requires four weeks before the red colour becomes discernable.” Dr. Harrington says in his Thoughts on Air, p. 30. “Mr. Kirwan, in opposition to Mr. Scheele’s experiments of the nitrous acid being phlogisticated from exposure to the sun, says in his notes upon Mr. Scheele’s book, p. 230.” “By pure nitrous acid, the author means the dephlogisticated colourless nitrous acid. I have exposed this in a phial half full, and closed with a glass stopper, to the solar light, and in a quarter of an hour found it phlogisticated; but when the phial was quite full, this did not happen, though I exposed it two hours to the same light. It appeared to me in the first case, that the vapour only

only was acted upon, being changed from white to red ; this vapour was absorbed by the liquor which then became greenish. In this case, the vapour may have taken phlogiston from the air, whose capacity for containing fire was increased ; this I am the more inclined to suspect, because the more of the bottle was left empty, the deeper green the liquor appeared to me to assume."

" But unfortunately for this theory ; instead of the air being better, (which it must have been agreeable to this opinion, and likewise the general one of injured air being phlogistified) upon examination it has become highly noxious."

Dr. Priestley says, Philos. Trans. vol. lxxix. p. 441. " My friend Mr. Kirwan, however, having always suspected, that the *air* was a principal agent in the business, I at this time gave particular attention to this circumstance; supposing that, if any part of the common air had been imbibed, it must have been the *phlogistified*, and that it was the phlogiston from this kind of air which had phlogistified the acid. The real result, however, was not so much in favour of this supposition as I had expected ; for the principal effect of the process was the emission of dephlogisticated air, so that the acid seems to become what we call *phlogistified*, by parting with this ingredient in its composition."

The reader must excuse my not following Dr. Priestley through all his calculations in this

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paper, as I do not think that his hypothesis, and mode of reasoning upon it, deserve such attention. He now leaps to the opposite ground, and supposes that the vapour emitted from the heated acids, is dephlogisticated air; and he endeavours to prove it by calculations, which are equally as erroneous, being upon as wrong a *data* as his hypothesis. In this case there is an exact conformity between them.

But the Doctor, who is indefatigable, (this is one of his chief talents as an author) produces another paper to the Royal Society, in favour of his singular hypothesis. And here I would observe, that the Doctor must exert himself in behalf of his hypothesis, as the opinion, that the acids are formed of different airs, is principally founded upon the doctrine of different airs entering into the composition of water: Therefore, as he himself has, after Dr. Harrington, endeavoured to destroy that hypothesis, the other must stand upon unsafe ground. But what is it Dr. Priestley cannot do? All must bow to his superior talents: Therefore, confiding in this superiority, he sends the Royal Society another paper, which we shall examine. He begins, p. 289. "In my late experiments on the *phlogistication of spirit of nitre by heat* it appeared, that when pure air was expelled from what is called dephlogisticated spirit of nitre, the remainder was left phlogisticated. This I find abundantly confirmed by repeating the experiments in a different manner, and on a larger scale; and I have applied the

the same process to other acids and liquors of a different kind. From these it will appear, that oil of vitriol and spirit of nitre, in their most dephlogisticated state, consist of a proper saturation of the acids with phlogiston, so that what we have called the *phlogistication* of them, ought rather to have been called their *super-phlogistication*.

" I began with treating a quantity of oil of vitriol as I had done the spirit of nitre, viz. exposing it to heat in a glass tube, hermetically sealed, and nearly exhausted; and the result was similar to that of the experiment with the nitrous acid, with respect to the expulsion of air from it, though the phlogistication not appearing by any change of colour, I did not in this method ascertain that circumstance. The particulars were as follow:

" After the acid had been made to boil some time, a dense white vapour appeared in quick motion at a distance above the acid, and tho', on withdrawing the fire, that vapour disappeared, it instantly re-appeared on renewing the heat. When the tube was cool, I opened it under water, and a quantity of air rushed out, though the acid had been made to boil violently while it was closing, so that there could not have been much air in the tube. This air, which must therefore have been generated in the tube, was a little worse than common air, being of the standard of 1.12 when the latter was 1.04. I repeated the experiment

periment several times, and always with the same result.

" That this air should be worse than common air, I cannot well explain. But in my former experiments it appeared that vitriolic acid air injures common air; and that in proportion as pure air is expelled from this acid, and the remainder becomes phlogisticated, or charged with vitriolic acid air, clearly appeared in the following experiment.

" Making a quantity of oil of vitriol boil in a glass retort, and making the vapour pass through a red-hot earthen tube, glazed inside and out, and filled with pieces of broken tubes, I collected the liquor that distilled over, and found it to be the same thing with water impregnated with vitriolic acid air. The smell of it was exceedingly pungent, and it was evident, that more of this air had escaped than could be retained by that quantity of water. The oil of vitriol used in this process was 1 oz. 9 dw. 18 gr. and the liquor collected was 6 dw. 12 gr. When I collected the air that was produced in this manner, which I did not do at this time, it appeared to be very pure, about the standard of 0.3 with two equal measures of nitrous air.

" At another time, expending 1 oz. 11 dw. 18 gr. of oil of vitriol, of the specific gravity of 1856 (that of water being 1000), I collected 19 dw. 6 gr. of the volatile acid, of the specific gravity of 1340, and 130 oz. measures of dephlogisticated

phlogisticated air of the purest kind, viz. of the standard of 0.15.

" It is easy in this manner to collect a great quantity of dephlogisticated air; but the principal objection to the process is, that after using a few times, the earthen tubes become tender, and too easily break, especially in heating or cooling. It is also difficult to lute the retort containing the acid and the earthen tube. The air produced in this manner is filled with the densest white cloud imaginable.

" Going through the same process with spirit of nitre, the result was in all respects similar, but much more striking, the production of both dephlogisticated air and phlogisticated acid vapour being prodigiously quicker, and more abundant. Expanding 5 oz. 8 dw. 6 gr. of spirit of nitre, I collected 600 oz. measures of very pure dephlogisticated air, being of the standard of 0.2. I also collected 1 oz. 7 dw. 14 gr. of greenish acid of nitre, which emitted copious red fumes. All the apparatus beyond the hot tube was filled with the densest red vapour, and the water of the trough in which the air was received, was so much impregnated with it, that the smell was very strong; and it spontaneously yielded nitrous air several days, just as water does when impregnated with nitrous vapour. Perceiving the emission of air from the water, after it had stood some time, I filled a jar containing 30 oz. measures with it, and without any heat it yielded 2 oz. measures of the strongest nitrous air."

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Does this require a comment? My reader, if he remembers what I have already said, may anticipate me in my explanation. The vitriolic acid becomes phlogisticated, yet at the same time discharged an air worse than common air, which, say they, contains three parts of phlogisticated, to one of dephlogisticated air. A most ridiculous explanation: For, if vitriolic acid air injures common air, why should it injure its own dephlogisticated air it had just parted with? Besides, if the vitriolic acid injured the air, the acid would become dephlogisticated; but, agreeable to Dr. Priestley, it becomes highly phlogisticated in the process; and more particularly the nitrous acid, which has considerably a greater power of injuring common air, gives out, in this same process, pure air which is not injured; though it is likewise in contact with the nitrous acid, and its vapour is the same as in the process of the vitriolic acid.

I need, I think, scarcely mention, that the strong acids saturate themselves with a great quantity of fire, and become phlogisticated; that the nitrous acid becomes so highly phlogisticated, as to emit a rich vapour or phlogisticated air, (erroneously so called, but I must give them their names) being the acid vapour highly saturated with phlogiston. That this air is produced from the acids by heat, Mr. Scheele found in the distillation of the nitrous acid. And I will inform Dr. Priestley of something, which very probably he does not know.

know. If, upon exposing these acids to great heat, he chuses to examine this vapour or air in the different stages, he will find it at first, what he calls phlogisticated: But, by still going on with the process, it will get so great a saturation of phlogiston, as to form his dephlogisticated air. As to the acids passing through hot tubes, and forming air of different saturations of phlogiston or fixed fire, which is his other experiment; it is the very same as that of the aerial acid or fixed air. This air, by having the electric fire pass through it, will form different saturations; viz. phlogisticated, dephlogisticated, and inflammable airs: Or heat, acting upon it when united to calcareous earths, will do the same. If this explanation is not deemed sufficient, we can make it still more evident. For, by adding spirit of wine or the essential oils to the nitrous acid, we can make it form all these different states of phlogiston; viz. dephlogisticated airs and the phlogisticated acid vapour. But had Dr. Priestley condescended to attend to what Dr. Harrington says, he could not have been at a loss to account for these phenomena. Dr. Harrington says in his *Thoughts on Air*, p. 309. "As we have all along proved the identity of the electric matter with phlogiston, it being allowed by all chemists, so we shall shew its effects upon different bodies. Mr. Henry in his preface (p. 14.) to the translation of Mr. Lavoisier's essays, says," "Dr. Priestley having mentioned his having formed sulphur by the union of inflammable

flammable air with vitriolic acid, as a proof of the identity of that air with phlogiston; he adds, that he had also proved, more unexceptionably than before, that the electric matter contains phlogiston, by making it to pass through the air, confined by the acids, in a syphon." "When," says he, "I use the dephlogisticated marine acid, the air is diminished by the process, and dephlogisticated. If I use the phosphoric acid or the phlogisticated alkali, the air is first diminished, and then increased by an addition of inflammable air. If I use VITRIOLIC ACID or the NITROUS ACID, there is a production of DEPHLOGISTICATED AIR, faster than the electricity can injure it."

"In the first instance the marine acid being dephlogisticated, will naturally attract phlogiston, therefore the air is decompounded.

"In the second instance with the phosphoric acid, or the phlogisticated alkali, the electric matter passing through them, there is a generation of inflammable air. We have shewn that the phosphoric acid has so great attraction for fire or phlogiston, that when it is exposed to a great heat, phosphorus will be produced; that, when united with the calx of lead, they will produce inflammable air. And the connection between the alkalies and inflammable air is well known. The electrical matter will turn alkaline air inflammable.

"The third instance of the vitriolic acid and nitrous acid, generating dephlogisticated air with it, may be easily accounted for. I suppose

pose it will not be disputed that the electrical matter reduces the calces of metals by giving them phlogiston ; that it forms alkaline bodies into inflammable air by giving them phlogiston ; and likewise the phosphoric acid into inflammable air from this cause.

" These facts being granted, can it be supposed that it can form the vitriolic and nitrous acids into air by dephlogisticating them ? One of the acids to be formed by it into inflammable air, and the other into dephlogisticated air, are such paradoxes as cannot be admitted. No, they have all received phlogiston, only the phosphoric acid has received a higher impregnation. This is demonstrable ; for if this air that is formed from the nitrous and vitriolic acids in this process be exposed to the nitrous acid, the air will be decompounded, and the acid strongly phlogisticated ; nay, by a peculiar nitrous and vitriolic acid, I have, by continuing the process for a long time with the electrical matter, formed them into an inflammable air. These are such obvious facts, as must force conviction. Chemists have attempted to account for these phenomena in a very vague manner indeed. Mr. Henry says, p. 15." " Now, from whence can this dephlogisticated air proceed, but from a decomposition of the acids by the phlogiston of the electric matter, in which the pure air is separated, while the phlogiston, combining with the remaining part or basis of this acid, forms a sulphur ?"

" To suppose that these acids, by receiving phlogiston, should produce dephlogisticated air, is a most singular supposition indeed. If we add phlogiston either to the nitrous or vitriolic acids, we produce nitrous and vitriolic airs. But is it possible that phlogiston being added to one acid could make it part with dephlogisticated air, and cause the other to part with inflammable air? Then, agreeable to this theory, the nitrous and vitriolic acids are formed of empyreal air, and the phosphoric acid of inflammable air. But in burning sulphur, pyrites, &c. Mr. Lavoisier found a quantity of empyreal air absorbed, and they turned to the vitriolic acid. Therefore, agreeable to this theory, if you burn phosphorus there is an equal quantity of empyreal air absorbed, (which he acknowledges himself;) but then the phosphoric acid with this electrical matter should yield empyreal air, as well as the other acids, if this be true. However, it yields an air, which they say, is the opposite to it, viz. inflammable air.

" There is just the same objection to their theory of the acids forming airs with earths. Mr. Lavoisier supposes it is by the empyreal air being set loose. Then, how comes some acids, viz. the phosphoric, the vegetable, the aerial acid, with these bodies to form inflammable and phlogisticated air; and the other, viz. the nitrous and the vitriolic with them to form empyreal air. Besides, in introducing this theory, we destroy Stahl's great doctrine of phlogiston,

phlogiston. But in short, chemists are really in a labyrinth with their doctrine of air, so that every new experiment gives a different hypothesis, and all the old rudiments are set at nought."

But what appears most extraordinary is, that Dr. Priestley, after he had been labouring to prove, that inflammable and dephlogisticated airs form water and the nitrous acid, should, with the same opinions, and hypothesis, endeavour to prove, that the nitrous acid is formed of phlogisticated, nitrous, and dephlogisticated airs \*.

In this mode of reasoning he proceeds upon no confirmed *data*, but only adopts the fanciful conjectures of the moment; and such are the theories of our modern chemists. Dr. Priestley has made experiment upon experiment, which compose many large volumes; and his opinions, taken from those experiments, have been continually

\* As nitrous acid is reproduced when dephlogisticated air is admitted to nitrous air; or, according to this theory, to dephlogisticated nitrous vapour and phlogiston, do not these principles, together with *water*, which is requisite, at least, to the forming of an union between them, make nitrous acid. If so, this acid must consist of dephlogisticated air, dephlogisticated nitrous vapour, phlogiston, and water. In the solution of metals in this acid, then, the first and last mentioned of these four elements, together with part of the third, must unite with the calx of the metal, while the second, joined with the remainder of the third (or which is the same thing with phlogiston from the metal) constitutes nitrous air. I do not need to add that *latent heat* seems to be a necessary ingredient in every kind of air. See Priestley's Experiments, vol. VI. page 411.

continually changing: Still in the dark, he could see no object distinctly. This induced him to follow an imagination unimpressed with the first rudiments of chemistry, and his great name has given authority to his conjectures.

It surprises me not a little to see Dr. Priestley change his ground, with respect to the process of respiration: This was the process upon which Dr. Harrington first attacked him. That chemist would have it to be a dephlogisticating process to the air; whereas Dr. Priestley maintained it to be a phlogisticated one. But the Doctor, who must be allowed to possess great penetration, finding his own ground untenable, shifts, like a wary general, to that of Dr. Goodwyn; yet would not, upon any consideration, suggest any thing concerning Dr. Harrington's hypothesis. However, I am afraid, that when this ground, I mean Dr. Goodwyn's hypothesis, comes to be fairly examined, it will be found equally untenable. I can scarcely think, that so penetrating and knowing a man as Dr. Priestley, can be serious in the choice of his present ground. Probably he has chosen it, through political policy, of obscuring the subject. Being so accustomed to conquer, he cannot support, even the idea of a defeat.

The Doctor begins with his experiments and arguments upon the burning of charcoal in atmospherical air, from which he estimates the quantity of dephlogisticated air turned into fixed air: But I shall enlarge no farther upon this  
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wrong *data*. My reader may see, page 72, that part of the fixed air comes from the charcoal.

The Doctor's next *data* is taken from his own respiration. He breathes a certain quantity of air; and by examining the air after respiration, he finds that both the dephlogisticated air, and great part of the phlogisticated air have disappeared. Hence he concludes, that a great part of the dephlogisticated air is received entire into the blood, only a small part of it being turned into fixed air. It is matter of surprise, that Dr. Priestley, after being so long conversant with aerial experiments, should bring these experiments to prove so singular an hypothesis.

In the first place, the Doctor is for making the life of animals act differently upon respirable air, to what the life of combustion does, and likewise putrefaction. But there is no fact in chemistry better established than this, that the air is acted upon by these processes in the same manner. Again, which is still more surprising! The Doctor makes phlogisticated air necessary to respiration; as a considerable greater quantity of it, than of dephlogisticated air, disappears in respiration. Here Dr. Blagden's explanation will no ways assist him to get out of the dilemma. The Doctor says, Philosophical Transactions, vol. lxxx. p. 110. "But, at the obliging suggestion of Dr. Blagden, I now think it more probable, that the deficiency of phlogisticated air was owing to the greater

greater proportion of it in the lungs *after* the process than *before*." But why is only phlogisticated air left in the lungs? Why not the fixed air, into which the dephlogisticated air must have been turned? Nay, why not dephlogisticated air? Indeed, that the lungs have not power to turn all the factitious dephlogisticated air into fixed air, has been fully shewn by Dr. Harrington, Mr. Scheele, and even by Dr. Priestley himself\*.

The Doctor, who is confined in his chemical opinions, to his experiments in gun barrels, &c. never takes a more extensive field. But if he will consider, that his theory supposes, that dephlogisticated air is the acidifying principle; and if such a quantity of dephlogisticated air is received every moment into the blood, what must a sheep's blood be, which is formed from the watery acescent vegetables, and with all this acescent air. What supplies its fat that it possesses in such great abundance?

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\* In the preceding experiments, and several others which I made about the same time, I found that mice would not live in dephlogisticated air till they had completely phlogisticated it, though they lived longer in it than, in proportion to its purity, with respect to common air; and for this I cannot assign any sufficient reason. I had once imagined that this was owing to my being obliged to make the mice pass through a quantity of water, by which the air was confined; but I put a mouse through the same water into a quantity of common air, and it lived in it till it was thoroughly phlogisticated. This may deserve a farther investigation. I should have put other mice into what remainder of the dephlogisticated air. See Priestley's Experiments, vol. V. p. 163.

That the air forms the red globules of the blood, is an undoubted fact: And likewise, that they are a high phlogistic body, is equally as well ascertained by the great Gaubius, and every other eminent physiologist. But to satisfy the Doctor by chemical experiments: If you take phlogiston in any form, either as an alkaline salt, oil, &c. and add them to the venous blood, they will immediately change it to the colour and qualities of arterial blood; the same as it receives when it comes in contact with the air. But if you apply an acescent body to the arterial blood, it immediately turns it to the colour and quality of the venous.

The Doctor talks of the dephlogisticated air carrying the phlogisticated air along with it into the blood, as if there was some strong attraction between them. But, in combustion, when the air is imbibed by sulphur and phosphorus, they are not imbibed together.

However, according to these experiments of Dr. Priestley, phlogisticated air is more necessary to respiration than dephlogisticated air, as more of the one than of the other disappears. But let the Doctor only try the experiment upon animals, which have *no hypothesis to establish*, and he will find very different results: We shall quote some of his own experiments. He says, vol. V. p. 161. "But to make the experiment in the most unexceptionable manner that I could contrive, I, in the next place, got two mice, of nearly equal size, and put them into exactly equal quantities, viz. about five ounce

measures, of the same dephlogisticated air (the measure of its purity, with two equal quantities of nitrous air, being 0.24) in nearly equal and similar glass jars, one standing in lime-water, and the other in common water. Both the mice continued in this situation something more than two hours and an half, after which the air which had been confined by lime-water appeared to be reduced in the proportion of 9 to 5 1-4 the measures of the test being 0.96; and the air which had not been confined by lime-water was diminished in the proportion of 9 to 6 3-4, the measures of the test being 0.98. Both the mice, though kept pretty warm, laboured alike with a difficulty of respiration, some time before I put an end to the experiment. In the course of it I agitated the lime-water a little now and then, in order to make it absorb the fixed air the better, by admitting fresh lime-water to the air that had been respired."

Mr. Scheele, by the respiration of bees, turned a large quantity of dephlogisticated air entirely into fixed air. See likewise Dr. Crawford's experiments upon the respiration of a pig: But neither the mice, the bees, nor the pig, had any hypothesis to establish; nor were their lungs so capacious as to secrete all these airs. You may, by laborious breathing, act upon almost any air. Mr. Scheele, whose knowledge of experiments was deficient to none, says, p. 160. "I filled a bladder with air produced from iron filings, and acid of vitriol, (No. 30, letter c.) and inhaled it in the manner

manner described before (No. 48.) I could inhale it no more than twenty times; and having again recovered in some measure, I again expelled the air from the lungs as much as possible, and then drew in again this inflammable air. After ten inhalations I found myself obliged to leave off, and found that it was no more inflammable, nor would it unite with lime-water; in a word, it was foul air."

The Doctor may soon know by an easy experiment, if there is any analogy between the life of animals and the life of flame, the latter turning dephlogisticated air into fixed air; and also, if he will reflect, that fixed air is fifty times more easily imbibed by water or moisture, than dephlogisticated air. I say, the experiment is easily made: Agitate fixed and dephlogisticated airs and water together, and see which the water more readily imbibes: That there is water and agitation in the act of respiration, particularly the latter, is what every one must be sensible of. The Doctor, I am sure, would breathe hard while attempting to establish his hypothesis.

I will now give you Dr. Harrington's explanation of this process, made public by him in 1780. His proofs are clear, and suited to the understanding of every chemist, that is not bewildered and infatuated with the new chemical doctrines, and the experiments of glasses, gun barrels, &c. He proves that atmospheric air, or at least the richest part of it, is formed of fixed air and water highly saturated

and neutralized with fire, or with the rays of the sun. That upon this air's being exposed to the blood; the blood, as having a greater attraction for the air's fixed fire, attracts it, and leaves the fixed air and the water in the expired air. He also proves that as there is a great deposition of water upon the lungs, so it is absorbed into the blood: And that the water carries a part of this fixed air along with it into the blood, which seems probable; because the water has fifty times a stronger affinity for fixed air, than it has for dephlogisticated air. Likewise, that the high factitious dephlogisticated air, is not so easily turned to fixed air. This is evident in the respiration of the mice just now quoted; therefore the mice could not live so well in it, as in atmospherical air, till they had injured it. If Dr. Priestley and other chemists will not bewilder themselves with fresh experiments, but only attended to Dr. Harrington's explanation of those already made, they would soon come to the true knowledge of what they are in quest of. Nay, they will attain to this, if Dr. Priestley and others but act up to what he himself says in the preface to his last vol. "Let us not, however, contend about *merit*, but let us all be intent on forwarding the *common enterprize*, and equally enjoy any progress we may make towards succeeding in it; and above all, let us acknowledge the guidance of that Great Being, *who has put a spirit in man, and whose inspiration giveth him understanding.*"

Dr.

Dr. Harrington intended to have given a full history of animal and vegetable life, with all the phenomena of the animal heat, muscular motion, glandular secretions, &c. The explanation how the air acts upon the blood, being only a preface to that work. But the treatment he has received, has checked his researches. His labours (to use his own words) instead of having met with that encouragement, which might incite him to promote the advantage of science, have been treated with neglect and insolence. Owing to this it is, that latterly his time has been employed, not in the investigation of nature's truths; but in the correction of modern chemical errors. I ring the alarm-bell to science and to truth.

The Doctor is now obliged to adopt the opinion that dephlogisticated air contains a great quantity of water. But had he attended to Dr. Harrington's experiments, he would have seen, that animal moisture is principally received from the air, upon its decomposition by the lungs; and likewise, that the moisture of expired air comes from the air, and not from the lungs. Dr. Harrington has shewn, in his Thoughts on Air, how this water of composition, as he calls it, is united to the acid and the fixed fire.

Dr. Harrington's system of airs did not originate from the experiments of gun barrels, &c. but from an accurate observation of nature. Take an egg, and examine its fluids; they are mild and bland, consisting of a watery *mucus*; but

but expose it to the air, and it will become highly putrid: Its fluids are changed from a pure state to one highly alkalescent and noxious, emitting a most nauseous stench. I need not enlarge upon this topic, as every one must be acquainted with the phlogistic alkalescent state of its putrid fluids. Then, how must the egg have received all this alkalescency? From the air, no doubt, as there was no other body that acted upon it. The air, according to their opinion, is highly phlogisticated; then the wonder still increases, as we have not only the phlogiston of the putrid egg, but also the phlogiston which the air has received, to account for. According to Dr. Priestley's explanation, one egg will phlogistigate two thousand gallons of air; therefore, agreeable to his experiments, this egg must have given to the air, one thousand gallons of inflammable air; or, according to Lavoisier, some ounces of charcoal; or, to others, an immense quantity of phlogiston.

My reader must excuse me, if I cannot bring myself to believe, that this fine, mild, bland lymph could possess so much phlogiston, as not only to turn it into a state so highly putrid and offensive, but even to phlogistigate such a quantity of pure air. And I beg leave to dissent from the opinion of those chemists who believe it; their reasons and chemistry being so very different from mine. To convince their judgment that they are wrong, is perhaps not in my power; yet I hope, I shall be able to convince their stomachs. Let those who hold the opinion

opinion I am combating, first swallow a sound egg, and then a putrid one, and I am sure their stomachs will be convinced, and of course their heads; the sympathy and relation between these two parts of the body, being so near and intimate: After this dose they would, I think, be of my opinion. Mr. Lavoisier is so much struck with the state of putrescency, that he wonders that chemists have not been more attentive to it: Some chemists have not passed it by; for, if he will please to attend to what Dr. Harrington says in his publication in 1780, he will there see the process of putrefaction fully demonstrated and proved. In the putrefaction of vinegar, the acid is turned alkalescent. See Dr. Harrington.

It will, no doubt, be expected that I should take notice of the Haerlem experiment of passing the electric spark through water. "They employed a tube hermetically sealed at one end, and the other opening into a reservoir of distilled water. At the sealed end a golden wire was inserted so as to project an inch and half into the tube. At the distance of five inches and one-eighth was another wire, which was carried through the open extremity: The first was connected to the prime conductor of a very powerful electrical machine; the other to the outer surface of a Leyden vial, the bottom of which communicated with the prime conductor, and which had a square foot of coating. As those wires, therefore, formed, by means of the water, the electrical circle, the spark was

was passed through the fluid; and soon after some very powerful shocks had been given, bubbles of air appeared in the water and gradually collected into large masses. When the column was so great as to extend to the end of the superior wire, the whole inflamed, and a very small residuum was left." Now the phenomenon is nothing else but this. The electric fire unites itself to a part of the water, forming an electrical vapour or cloud, the same as the phenomenon we see in the clouds. When the vapour is so considerable as to extend to the point of the wire, and come within the influence of the electrical fluid, it strikes the vapour or cloud, so as to break it with an electrical explosion; just as when a cloud is struck with the electrical fire, it will burst and explode.

Dr. Harrington has fully shewn how these phenomena take place in the explosion of inflammable and dephlogisticated airs. In the explosion, the fixed fire of these airs is set loose; the acid of the dephlogisticated air, and the water of both airs being in the residuum. If there is a great proportion of inflammable air, it (as he most satisfactorily shews) flies off with the acid, producing the explosion; but if in less proportion, it only decomposes the dephlogisticated airs into an acid and water. But for a more full explanation of all these phenomena, the reader may consult his Letter to Dr. Priestley and others, in which he will see

see these experiments discussed in a very clear and extensive manner.

The Royal Society have paid great attention to every experiment upon air: And indeed it is, and has been for some time past, a favourite subject of investigation all over Europe. I have given a regular detail of the different papers upon that subject, presented to the Society, since Dr. Harrington's Letter was published. Mr. Milner's paper comes now under our observation. He thought he detected the volatile alkali, by passing dephlogisticated nitrous air through a gun barrel. There is nothing wonderful in this: For, the effect of passing the nitrous acid and phlogiston through a heated gun barrel would be this; the acid then, having as it were an opportunity of saturating itself with more phlogiston from the iron, would be so concentrated as to give the alkaline smell. But this strongly confirms our hypothesis; viz. that alkalies are a concentration of fire; and that the volatile alkali is a high concentration, similar to inflammable air. Nay, it proves itself; as the volatile alkali may be formed into inflammable air; for Mr. Milner's dephlogisticated nitrous air, which produced the alkali, is formed from the inflammable air of the metal.

But, as our modern chemists are apt to wonder at any experiment they think new, so Mr. Milner calls the production of the nitrous air, by passing the volatile alkali through manganese in a heated gun barrel, a wonderful trans-

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mutation. He thought that the nitrous air was produced by the dephlogisticated air of the manganese and the phlogisticated air, which according to him, the volatile alkali was supposed to possess. However, the experiment is nothing more than this. The gun barrel heated by the charcoal, expels the dephlogisticated air of the manganese; and, upon the volatile alkali coming in contact with it in the red hot tube, sets them on fire; and the dephlogisticated air is, by the intenseness of the fire, decompounded into the nitrous acid. This will be the effect, if they are fired together out of the gun barrel; which is so evident, that none can possibly mistake it. The acid then, (as Mr. Milner judiciously observes) acts upon the iron, and forms nitrous air.

Now, as this experiment can admit of no other explanation than the one we have given it, let me ask Mr. Milner a few questions. Will not manganese, when exposed to the heat that he placed in it, yield dephlogisticated air? Will not this dephlogisticated air and the volatile alkali, when formed into a vapour, burn together? Will not the combustion, under that intense degree of heat from the gun barrel, produce nitrous acid? The answers to these questions, which may be easily given, are a sufficient explanation of the phenomena. Mr. Milner's explanation is a very curious one, founded upon the present theories of the day. For, if it were from the union of the dephlogisticated air, and the phlogisticated air of the volatile

volatile alkali, supposing it contained this air; and suppose these two airs would produce the nitrous acid, yet it would not have the effect he ascribes to it. To make the experiment according to his hypothesis: Let him pass the phlogisticated air, and not the volatile alkali, through the manganese, and see if they will produce the same phenomena. In this case, the phlogisticated air, being free and disengaged, should act more strongly upon the dephlogisticated air of the manganese: But that the volatile alkali does not possess any phlogisticated air, being only one uniform fluid, seems true, from the following observations: Upon its being burned, a part may be left not so highly saturated with phlogiston; just as in metallic earths, there is generally part left, which is not capable of being reduced: Even the inflammable air, from metals, has a small quantity of similar dregs. A thousand examples might be brought to prove, that this is the case with all bodies in nature. The atmospheric air is a perfect homogeneous fluid, the same as milk: One part of it only, when separated, consists of a richer fluid, similar to cream; but while undecomposed, it is, like milk, one homogeneous fluid.

I cannot help smiling at the present mode of experiments, and the construction of those experiments, that are at present adopted. Mr. Milner, in passing the vapour of the nitrous acid through a heated gun barrel, filled with iron filings, says, "First, nitrous air is formed,

then dephlogisticated nitrous air; and lastly, phlogisticated air." These changes to take place upon the vapour, while it is passing through the hot barrel, is a most sudden transmutation indeed; but such are their theories, and such their reasonings.

I read in the analytical review (just before the last sheet was printed) an extract from Mr. Westrumb's experiments \*. They very forcibly corroborate

\* Mr. Westrumb observed, that on plunging a spoon filled with cinnabar into dephlogisticated marine acid air, a copious white vapour arose. He afterwards introduced a stick of fir well covered with powdered cinnabar into the same kind of air, when a similar vapour arose, the extremity of the stick was covered to a coal, and on performing the experiment in the dark, *flame* was frequently visible. Thirty or forty inches of the air, taken at the latter part of the process in which it is made, was confined over water, and brought to a temperature of  $60^{\circ}$  or  $70^{\circ}$ . Opening it suddenly, and throwing into it thirty or forty grains of cinnabar, *a brisk inflammation arose*, accompanied with a suffocating vapour; and a portion of marine salt of quicksilver was found in the vessel, without the least *vestige of sulphur*. Thirty grains of sulphur being treated in the same manner, it was partly decomposed, but without any appearance of inflammation or vapour. Camphor likewise did not inflame; but a portion of it assumed an oily form. Oil of cloves was heated without undergoing any change. Oil of turpentine grew warm, and was converted into resin. Spirit of wine grew hot, smoked, but did not inflame, and acquired an agreeable smell, yet was not changed into æther. Magnesia combined with the gas without any heat arising. Aerated volatile alkali formed sal ammoniac, with heat, and some vapours. In all these experiments there was an absorption of gas. Golden sulphur of antimony was converted into white vapours, without inflammation, and produced butter of antimony: If introduced on the point of a stick of fir, the extremity of the stick was converted into coal. Kermes mineral inflamed with a clear redish

corroborate my principles of combustion, and that phlogiston is concentrated fire: For, tho' Mr. Westrumb made them with a different view, and to establish opinions in opposition to ours; yet, if my reader will carefully run his eye over that extract, he cannot but construe the experiments conformable to our hypothesis, that phlogiston is concentrated fire. Had I seen them sooner, I might have interwoven them into my arguments.

I beg

dish white light, accompanied with sparks, and butter of antimony was produced. Antimony produced a beautiful clear, white light, with sparks; the bottom of the glass became red-hot and the residuum was butter of antimony. Regulus of antimony gave the same results. Regulus of arsenic inflamed with a beautiful green and blue flame, and butter of arsenic was produced. Regulus of bismuth gave a clear, bright, bluish flame; and the residuum was marine salt of bismuth. Regulus of nickel burnt with a yellowish white flame, with sparks, and left marine salt of nickel. Regulus of cobalt gave a blueish white light, and produced a marine salt of cobalt, capable of making sympathetic ink. Regulus of zinc burnt with a beautiful white flame, gave fewer sparks than bismuth, and produced marine salt of zinc. Tin filings burnt with a weak blueish flame. For this experiment, and the two following, there should be no water in the vessel, and the gas should be of a deep yellow. Lead filings burnt with a clear, white, and sparkling flame. Copper filings gave a red flame. Iron filings burnt at the bottom of the vessel with a red light. In all these experiments marine salts of the different metals were produced. Forty grains of iron filings in eighty inches of gas, a small quantity of water being in the bottom of the vessel, burnt in the same manner, without detonating, or disengaging inflammable air: Yet inflammable air ought to have been produced, if it arise from the decomposition of water, and the base of vital air really constitute the difference betwixt common and dephlogisticated marine acid. Quicksilver did not enflame, but lost its fluidity, and was partly decomposed. In all the experiments mentioned

I beg leave to suggest one hint, I hope my treatment will not be the same as Dr. Harrington's; for, similar opinions to his have been given, without ever hinting by whom they were first given; nay, even in some cases delivered by others as their own.

I flatter myself that I have extended Dr. Harrington's theory of the aerial system, therefore I shall keep watch very tenaciously. But his theory must stand firm, and no depredations upon it can essentially injure it; there being no doubt but time will do it justice: But it is very hard to have that time so long procrastinated. He, from great labour and assiduity, and by strict and accurate observations and experiments in the different philosophical

mentioned since that with the Kermes mineral a large quantity of vapours of marine acid with a metallic taste, and disagreeable smell, approaching that of burnt horn arose. Aerated volatile alkali being first thrown into this gas, then an equal quantity of caustic volatile alkali, and afterwards a small portion of regulus of antimony, a brisk detonation ensues. Caustic volatile alkali produces heat, and white vapours. Two drams of caustic volatile alkali thrown at once into thirty or forty inches of gas, produce a red flame, resembling the aurora borealis: Sometimes a noise is heard, which might be mistaken for detonations, but it is only occasioned by the water reduced into vapour, for the heat is great: The produce is sal ammoniac. One part of the charcoal, and two of regulus of antimony, inflame like antimony. Thirty grains of mineral coal inflame in fourscore inches of this gas at a temperature of  $90^{\circ}$ . Many other substances are susceptible of inflammation in this gas.

From these experiments, Mr. Westrumb draws many conclusions in favour of phlogiston, and shows, that all the phenomena are not explicable on the antiphlogistic system. See the Analytical Review for December, 1790, p. 471.

phical departments, formed his present hypothesis, which has stood firm since he first gave it to the public: And though it did not come out with the pompous apparatus of modern chemists; yet, while their opinions have been as changeable as the wind, (for, upon the whole, there never was such a heterogeneous mixture of opinions; the only just ones being those which are the same as his) his stood firm as a rock; time, instead of impairing, has given them more strength.

He first shewed, by the most convincing experiments and arguments, that water makes an essential constituent part of atmospherical air; yet the idea was scouted. But now one of the first leading aerial chemists gives it likewise as his opinion, but without ever adverting to Dr. Harrington: Yet his present hypotheses of airs, which seem to change as often as the day, I will declare, with confidence, are a mass of strange absurdities.

It may be construed, that I have treated the opinions of some eminent men rather too cavalierly. The answer I make to that is, the severe treatment which Dr. Harrington's opinions have met with. I think no one who views his system, as being the true one, can look coolly upon the usage he has received. Not the least attention paid to his labours; but, in some instances, directly similar opinions given by others without once suggesting his name: But, I should hope it has been from inadvertencies; and that the same conduct will take

take place no more. With such a persuasion I shall now rest; and to confirm that, they must either acquiesce with Dr. Harrington's system; or, at least, give it a fair and open discussion: But whatever way they do, to do it publicly.

Before I bid adieu at this time to the public, I would ask a favour, (though in propriety of language it is no more than justice) that every friend of science would so far interest himself in the behalf of injured merit, as to allow a candid investigation of the principles laid down in this Treatise. The characteristic of this free, learned, and generous nation is, to do justice to men and things, and to make every literary character, however much exalted, amenable to their tribunal.



*This TREATISE will be published on March the 7th, 1791. My Reader will, no doubt, anticipate my reason for naming it here.*